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Biophysics

THE rise of biophysics is an expression of the fact that, more and more, physically trained scientists are becoming ensnared in problems involving living matter. It is, after nuclear physics, one of the most significant developments of modern science. Physics for many reasons has long been concerned primarily with inanimate matter. But we are now faced with the tragic realization that a large measure of understanding and control of our physical environment, intellectually satisfying and wealth-producing though it is, has not only failed to define the living state but has engendered human problems that endanger continuity of life itself. This is one of the most compelling reasons why physical science is moving toward a greater preoccupation with the animate half of nature.

Had biophysics developed a century ago it would probably soon have taken the form of a highly organized, if fragmentary, inquiry into the physics of biological material. But physics has lost much of its former universality, and it is not surprising that the emerging biophysics is amorphous and episodic, and may never acquire that unified elegant expression which made classical physics so aesthetically satisfying to the scientific intellect of its day. Nonetheless, biophysics already has its fundamental aspect and its workaday fields of application.

The oldest of these is in fact a branch of physiology. In part experimental and in part a mathematical biophysics, it seeks to account for the form and behavior of biological structure by means of the mechanics, optics, and electricity of classical physics. Its contributions to the mechanics of respiration and blood flow, and to our knowledge of how a nerve acts or an eye functions, are well known.

Physics now provides many radiations besides light and heat to which living matter may be exposed, and research into their generally injurious effects is building a second chapter of biophysics. They are useful in the control of disease, and their analysis throws some light on vital processes; we all know that knowledge of these effects is needed as protection against the destructive agents that man has himself called into being.

Physics cannot be satisfied until it has interpreted the properties of matter in terms of its atomic and molecular composition. The molecules of living matter have a complexity not found in inanimate nature. New methods for dealing with them lead to a third kind of biophysics which seeks, in combination with biochemistry, to understand properties of organic matter in terms of the atomic make-up, the reactions, and the arrangements of these giant molecules.

Underlying biophysics is a search for a scientifically adequate definition of the term *alive*. All interpretations of nature use intuitive concepts which mark the present limits of our intellectual perception; and *alive* involves such a concept. Its scientific indefiniteness is immediately apparent when, for instance, we ask if viruses are alive. Information that will make our definition more precise is coming from many sources, but there is a widespread feeling that the study of viruses—those denizens of a seeming twilight zone between the living and the nonliving—can supply much of it.

Evidently biophysics in its essence is more than either the use of physical tools by the biologist or the application of physical precision of thought to problems of biology. Law in physics has been established with the aid of simple systems manipulated by the experimenter. Although many of the attributes of living matter can be analyzed through such systems, life is a coordinated activity of entire organisms never fully under the experimenter's control. The alteration in physical thinking needed to deal with this situation may prove to be the most important consequence of our growing concern with living matter.

RALPH W. G. WYCKOFF
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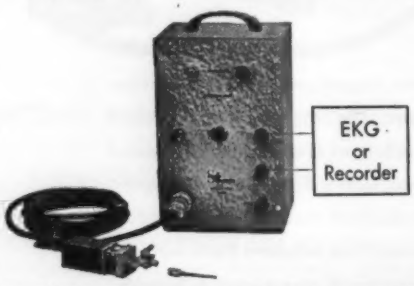

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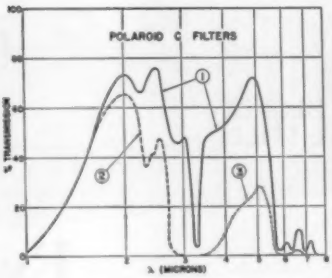

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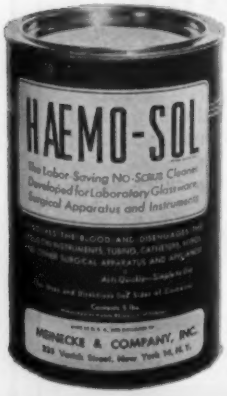

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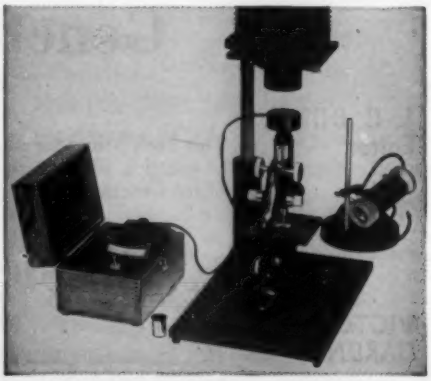

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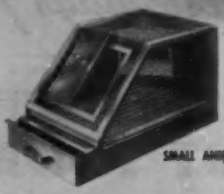

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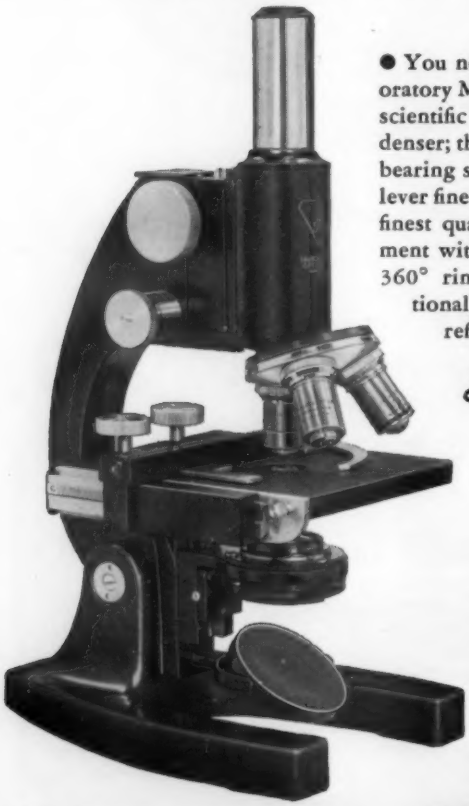

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Science in Bondage[1]

H. J. Muller

Department of Zoology, Indiana University, Bloomington

UNLIKE THE OTHER MAJOR CULTURAL ACTIVITIES OF MANKIND, science is a relative newcomer, almost an intruder on the human scene. Many so-called primitive peoples have for ages had highly advanced arts of both visual, sonic, kinesthetic, and verbal types, and have been masters of elaborate techniques for arousing human emotions, for creating individual and mass anger, fear, awe, or ecstasy, and for instilling given attitudes of mind, and given systems of beliefs. This is not true of science. In our present-day sense of a systematic search for truth, based on the pooled observations, experiments, reasonings, and counter-criticisms of numberless persons who have tried to free their judgment of any but objective criteria—in this sense, science has been almost nonexistent throughout the long history of *Homo sapiens* until we come to Greece and her outposts. Even there it flickered rather spottily. It is only since the rekindling of this small flame by medieval Arabs, Persians, Moors, Jews, and then Italians, followed by its gradual fanning into a full flare with the rise and dissemination of Western civilization during the past five centuries, that science has acquired a widespread and secure existence—or, at least, one that has *appeared* to be secure.

Nevertheless, there is ample evidence that the *ability* to think scientifically has long been widely diffused among the peoples of the earth. This is clearly shown by the mathematical and astronomical achievements of several ancient peoples, especially the Babylonians, Mayas, Hindus, Chinese, and Egyptians. It is also illustrated by the engineering, the metalworking, and other material techniques developed by some Central Africans and Peruvians, and by the various agricultural methods that arose independently in all the seven great centers of origin of cultivated plants delineated by Vavilov—Ethiopia, China, India, Iraq-Iran, Peru, Mexico, and the southeastern Mediterranean-Caucasus area. And today individuals of all "races" have shown themselves capable of making major contributions to science as we know it. This obvious possession by peoples in general of the potentiality for successfully pursuing science, coupled with the marked restriction of science as such in human history, shows what an extremely delicate cultural plant science is. Let us then inquire into the nature of the highly special soil and climate that its continued growth demands.

The growth of science (and without growth it withers) does not depend only on a few great discoveries. It depends equally on that slow accretion of multitudinous small steps that furnish the bases for, and the necessary extensions of, those discoveries, and also on the correction of the even more numerous missteps continually being made. It is evident that to carry out all this painstaking work, as well as to provide a body of personnel large enough for the origination of even a few major scientists, a very considerable number of scientific workers is necessary. This means not only a large educated public, but a large economic surplus. In most societies of the past in which a large surplus has existed, however, it has come almost exclusively into the possession of people who made leisure a profession, and whose ideal was "conspicuous consumption," as Veblen has termed it. The development of science, on the contrary, requires that much of the economic surplus become available for those groups who do the actual work of the world, and who have an ideal of constructive achievement.

The attainment of such a distribution of the products of labor and the existence of such an ideal imply that, in the society in which science develops, a large proportion of the so-called common people has a high standard of living, highly developed techniques, and considerable education. This means, too, that they have an effective voice in the management of their own affairs and participate in decisions affecting the whole community. Along with this comes a sense of their own dignity, as well as a basis for believing that they can still further improve their situation, both materially and culturally. All these factors together form part of the groundwork that is necessary before the "common" man and woman can produce individuals with that controlled initiative and creativeness that characterize scientific activity. Such conditions cannot exist long or securely in any community founded upon slavery, nor in one resting upon the work of oppressed and poverty-stricken masses who live on a bare subsistence level. In other words, a rather high level of democracy is necessary—one that, until recent times, has rarely been found in communities that have evolved beyond a primitive stage of development.

The community that nourishes science must not only be free from the physical oppression of nature and of other men, and have grounds for hope in its own prospects; it must also be relatively free from the despotism of imposed ideas. Its history must have entailed processes of intellectual disequilibration that led men to doubt the dogmas of their forefathers. Rarest of all—into the breach thus made there must

[1] Based on an address delivered at the panel on "Science and Totalitarianism" of the Congress for Cultural Freedom, Berlin, Germany, June 27, 1950.

have entered, in considerable strength, not merely a new and equally rigid set of dogmas, but a spirit of inquiry and objectivity, a wide tolerance for objectively reached but conflicting conclusions of others, a custom of candid criticisms, a distrust of all argument by authority and of all wishful thinking.

Western civilization has approached these conditions only by dint of centuries of struggle toward higher material techniques, toward ever-wider and more potent democratic procedures, and better popular education. At the same time, following contact with other cultures, including those represented in the rediscovered literature of the ancients, men were led to question further and further the voice of ancient tradition, as well as that of contemporary authorities. Moreover, travel brought men new knowledge and new problems, as well as fresh opportunities for gain and adventure. In all these ways, then, the stage has been set for the rise of science, but this rise has only been at the cost of constant effort, and contention with both man-made and natural obstacles.

Even yet, the very findings of science that are of the greatest significance for a deeper understanding of ourselves and of the universe are the most apt to arouse concerted opposition from powerfully organized groups representing established ideologies and institutions that the new knowledge would upset; hence, even in Western civilization, persistent vigilance and endeavor are necessary in the defense of the honest search for truth and of the teaching of it. It is one of the greatest strengths of this civilization that, viewed over the course of the centuries, it has made marked progress in this respect and is so constructed that the efforts of enlightened men to push this progress still farther *can* have a considerable measure of success.

In the profound discouragement and paralysis following the wreckage of war, however, some have been misled by the lure of complete despotism, masquerading under the name of "national socialism" or "international communism." Where one of these movements has gained control, it has become clear only too late that modern technology, both material and social, affords the tyrant a far more inescapable, efficient, and personalized grip over his unhappy subjects than any ancient or oriental tyranny. Under such conditions science—indeed, all culture—becomes a trained bear with a ring through his nose. At the tweak of his master's hand he must try to dance, but his virility is gone, he languishes away his life in desperation, and his days are numbered.

An audience in Berlin knows only too well the disillusionment with regard to progress in science and in cultural life in general that followed the accession of Hitler, with its disastrous purges on grounds of race and politics and with its deadening regimentation of ideas. My own branch of science, genetics, was the most perverted and outraged of all, since in its place a tissue of lies was fabricated in support of the dictator's racist psychosis. This would have been used to justify a state of world slavery in which, had it continued, all fundamental science must at last have perished.

To many an outsider it seems amazing that any persons who have suffered such disillusionment and loss as those who experienced Nazism should be ready to turn again to a dictatorship. But the Soviet dictatorship claims to be the very antithesis of Hitler's, and it presents a very different façade, reflecting the great differences in its earlier aims. It pretends to be a movement of liberation, as indeed it was originally intended to be. In the first flushes of release that followed the demolition of the semifeudalistic tsarist system it did in fact afford the people, not more food, fuel, or clothing, but more voice over their conditions of work, more education, more right to live and think as they wished to, and more hope. With this went a great upsurge of culture, including science.

But gradually, secretly, by hook and by crook, as well as openly at the point of guns, the Communist Party, claiming to represent the common man, took more and more power out of his hands, ostensibly for his own good, until there was no check left on the power of the Party. In each place of work, whether economic or cultural, an inner Party cell is planted to direct and redirect activities. Moreover, by its false accusations, its condemnations and dismissals of individuals, usually based on jealousy, and its glorifications of others who play its game, it holds workers and intellectuals alike in fear and subjection. At the same time, within the Party itself all vestiges of democracy have sedulously been rooted out.

Thus the entire Soviet system has become like a gigantic spiderweb. All its radii—political, military, economic, social, cultural, educational, and scientific—have been brought under the absolute control of the one center and of one omnipotent being who sits grasping all these strands at their junction. In this structure, all directives proceed outward, and peripheral criticism of more central decisions is never allowed. Neither the central group nor its innermost member, however, is characterized by high cultural development, or by an appreciation of the methods or spirit of objective science. Yet their pronouncements on matters of science and culture are hailed as revelations, even as were those of Nero.

For the security of a tyranny so unparalleled in its thoroughness it was important for all expression of thought to be brought under the complete domination of the Party. For this purpose Party organs were established for the control of all instruments of propaganda, education, and communication—the schools, press, film, radio, organizations of scientific, professional, trade union, and social character, sports, celebrations, and public assemblages. As a second check, a vast secret intelligence system was set up parallel to all this, and espionage was inordinately expanded and intensified. To render the spying still more effective, a new concept of morality was inculcated, making it a man's first duty, above all personal ties, to report

any signs of dissatisfaction with the Party or with its leader that might be shown even by the closest friend or relative. On top of all this were superimposed the great mass arrests and "trials," which gained such momentum in the thirties. These not only provided a mobile labor force of 10–20 million out-and-out slaves, but reduced the remaining population to abject submission in all intellectual, as well as physical, spheres of life. Thereafter, conformity must be complete, in deeds, in words, and even in facial expressions. Could any setting be less propitious for the encouragement of that adventurous thinking in new directions that must characterize fundamental science?

As an apologetic for all Soviet policies and doctrines, whether in science or elsewhere, recourse was had to the heterogeneous collection of notions on matters of science and philosophy, largely borrowed from Hegel, which Marx and Engels had long ago put together under the title of "dialectical materialism." Although it embodied some important advances over earlier views, its artificial schemes do not correspond with the operations of nature as we have now come to know them. Moreover, so far as natural science is concerned, this view is especially at fault in maintaining that all reactions in nature are basically a struggle between "opposites." This is a transparent attempt to make natural processes resemble that antiquated concept of social processes according to which the class struggle is always the prime mover in human progress. In this way, conflict and hate are made to lie at the bottom of all good.

Today dialectical materialism is no longer even an earnest attempt to interpret natural and social changes. It has been frozen into a medieval scholasticism. On the one hand, it is reduced to mystical and unintelligible slogans that are taught by rote to hundreds of millions in place of the credos of the church. On the other hand, since the mode of application of these doctrines for the reaching of specific conclusions is seldom clear and may lead to the most divergent results, official interpreters have been trained to twist the doctrines so as to provide "philosophical" justifications, as wanted, for any opinions, policies, or programs that happen to be favored by the central power. Thus, dialectical materialism has become a club, wherewith dissident opinions are condemned as undialectical or unmaterialistic and therefore anticommunistic, and persons holding such views become branded as traitors and enemies of the people. And if it still remains too obvious that the condemned views are founded on facts and logic, as has been the case with genetics, then there is another club in reserve. The views are labeled objective, in quotation marks, instead of partisan, class views, and it is proclaimed that the "most important principle in any science is the party principle."[2]

Whereas in some branches of science all these conditions have simply worked to undermine the spirit of the scientists, and to retard and deflect them in their work by the action of numerous, individually minor restrictions and disturbances, in other branches there have been systematically organized frontal attacks, controlled by the center, upon the main scientific principles and the main personnel. In the case of genetics, I happen to know much of the history of these attacks at firsthand and will draw upon it to illustrate the fate to which science in bondage is likely to fall victim.

Genetics, the science of heredity, variation, and evolution, had attracted a considerable body of able young Russian scientific workers shortly after the Russian revolution. This new science had itself been revolutionary in its discoveries by establishing the existence of units of living matter, called the genes, far smaller and more fundamental than the cell itself. These genes serve to carry the biological heritage from one generation to another, according to an amazingly precise set of principles. And these same genes, when they undergo changes, or mutations, give rise to the potentiality of biological evolution that Darwin had seen at a distance, as it were. It was found, quite definitely, that changes in the genes are usually caused by accidental molecular events, like those occurring on application of heat or of x-rays. Such changes do not correspond with the alterations that exercise, nutrition, or other environmental conditions induce in the body that carries the genes. In other words, Lamarck's old doctrine of the inheritance of acquired characters, which antedated Darwin's theory of evolution by the natural selection of accidental changes, was proved erroneous.

Realizing the significance of this fundamental subject for an understanding of the nature and history of living things, and of the future possibilities of mankind, as well as for the improvement of agriculture so needed by the USSR, Russian scientists soon forged their way into the front rank in this field. They became conspicuous among those who helped make it the most exact and well documented of all the biological sciences—the field most nearly comparable, both in the precision of its methods and in the reliability of its conclusions, with the sciences of physics and chemistry.

Beginning in 1936, however, a series of attacks upon genetics was instituted. These ranged from deliberate misrepresentations and vilifications in the press to forced "confessions" of error and guilt from some of the leading geneticists, followed by their disappearance and the closing of their laboratories. These men, as some of them whom I knew well explained to me, made their false "confessions" out of loyalty to the Party that ordered them to do so. That did not save them, however.

About the same time, because no real scientist could be found to attack genetics, this assignment was delegated to a half-educated and paranoic young demagogue named Lysenko, who had done some work in raising plants, but who was in fact ignorant of sci-

[2] From an editorial in *Pravda*, August 27, 1948.

entific principles and incapable of understanding them. Lysenko's reputation was systematically inflated before the public eye, and he was provided with a sophisticated interpreter of dialectical materialism, a cynical weaver of words named Present, so that Lysenko's crudities might be disguised and served up to the public as profundities. After Lysenko and Present had been given some preliminary practice in decrying genetics and in influencing farmers, and had been furnished with a band of mercenaries from the farms, who posed as scientists also but were still more ignorant, a kind of public gladiatorial combat between Lysenkoists and geneticists was arranged by the Party in December 1936. Although it was apparent to the hundreds of scientists in the audience that the contest was one between science and bigotry—and they showed this by the distribution of their applause—nevertheless, the politically appointed moderators of the meeting, in summing up the debate, censured the geneticists, and in the public press it was made to appear as if the latter had come out poor seconds.

Having been thus castigated and weakened, the geneticists were thereafter subjected to a continuous sniping process and to two more staged tournaments, one in 1939 and the last in August 1948. By the latter date all the noted names of Russian genetics had disappeared; the great Vavilov had perished in a labor camp in Siberia, and many others whose memory I hold dear had lost their lives in unexplained ways. Thus only a feeble remnant of comparative weaklings in the science was left to defend or compromise it. The show of 1948 was settled when, after the last word for genetics had been said, Lysenko made the smug announcement that the Communist Party had approved his position in advance and had declared the geneticists wrong. According to *Pravda's* accusation, they had been "objective," they had forgotten "the party principle" in science. But, of course, the Party in its greater wisdom concerning matters of science was able to override them.

Following this exhibition of barbarism there were, of course, recantations and apologies, and the Academies of Sciences, of Medicine, and of Agriculture all sent thanks to Stalin for his personal guidance in bringing this great reform about. At the same time, the laboratories of genetics were closed, their remaining workers were somehow disposed of, courses on genetics were abolished, and all books on the subject were banned. This was only the beginning, however. The principles of genetics are essential to all modern biology, so it was necessary to extirpate them from all curricula and publications dealing with biological subjects. Texts had to be rewritten, and staffs of colleges and of scientific publishing houses had to be purged. In various other lines of biology, medicine, and agriculture, world-famous scientists were dismissed or disgraced merely because, although they were not specialists in genetics themselves, they had refused to renounce its principles.[3]

[3] For details of the Communist Party's attack on the science of genetics, the reader may be referred to the following books: P. S. Hudson and R. H. Richens' *The New Genetics in the Soviet Union* (1946); Julian Huxley's *Heredity East and West* (1949); J. Langdon-Davies' *Russia Puts the Clock Back* (1949); Conway Zirkle's *The Death of a Science in Russia* (1949); and, finally, *The Situation in Biological Science, a Verbatim Report of the Sessions of the Lenin Academy of Agricultural Sciences, July 31–August 7, 1948*, published separately in German and in English by the Foreign Languages Publishing House, Moscow (1949). In the last-named book (International Publishers, New York), the Russian administrators stand self-condemned. Their temerity in publishing these otherwise incredible proceedings shows how far they now are from a realization of what is meant by science or, indeed, by rational thought itself. Among articles dealing with the subject, see E. Ashby's "Genetics in the Soviet Union" (*Nature*, Dec. 11, 1948); Robert C. Cook's "Lysenko's Marxist Genetics" (*J. Heredity*, July 1949); C. D. Darlington's "A Revolution in Soviet Science" (*J. Heredity*, May 1947); and "Science Rejects Dictation" (*New Leader*, Nov. 26, 1949); Th. Dobzhansky's "Lysenko's Genetics" (*J. Heredity*, Jan. 1946); and "N. I. Vavilov, a Martyr of Genetics" (*J. Heredity*, Aug. 1947); R. B. Goldschmidt's "Research and Politics" (*Science*, Mar. 4, 1949); J. S. Huxley's "Soviet Genetics: the Real Issue" (*Nature*, June 18 and 25, 1949); S. Kaftanov's "In Support of Michurin's Biological Theory in Higher Institutions of Learning" (*Science*, Jan. 28, 1949); J. Langdon-Davies' "The Russian Attack on Reason" (*Fortnightly Rev.*, May 1949); I. M. Lerner's "Genetics in the U.S.S.R., an Obituary" (Pub. Univ. Brit. Columbia, Lecture Ser. 8, 1950), H. J. Muller's "The Destruction of Science in the U.S.S.R." Pts. I and II (*Saturday Rev. of Lit.*, Dec. 4 and 11, 1948); H. H. Plough's "Bourgeois Genetics and Party-Line Darwinism" (*Am. Scholar*, Summer 1949); T. M. Sonneborn's "Heredity, Environment, and Politics" (*Science*, May 19, 1950); Bertram D. Wolfe's "Science Joins the Party" (*Antioch Rev.*, Spring 1950), and the May and August–September 1949, issues of *Bull. of Atomic Scientists*.

The same campaign was soon after carried into all satellite countries, where geneticists have been terrorized or forced into other lines of activity. The death of my old friend Kostov, well-known geneticist and Minister of Agriculture in Bulgaria, occurring during a genetics purge, was announced in 1949. Throughout China a comparable liquidation of scientists in this field has taken place. As for the Soviet zone of Germany, the situation will surely be understood by those in Berlin who have followed the controversy on the subject in the local press.

For a long time it has been evident that genetics has been distasteful to the very center—that is, to Stalin himself. We might speculate at length on the reasons and still we could not be sure, for the interpretation would turn chiefly on matters of personality. (What a reflection on the doctrine of the economic determination of history, under a totalitarian system!) Whatever its causes, we can be sure of the consequences of destroying a basic field of science. As all science is becoming more closely interdependent, it must inevitably have vitiating effects in many other theoretical fields. At the same time, practical progress in agriculture and animal husbandry will be greatly retarded. In fact, many previous achievements—such as Vavilov's invaluable world collections of cultivated plants, the basis for the construction of improved combinations—have already been lost.

It is especially to be noted that, in the sphere of the bearing of biology on theories of the nature and potentialities of man, the Communists have lost the scientific basis for answering the pernicious racist doctrines of the Nazis. They have thrown overboard

the geneticists' findings that the great changes wrought by differences in environment on living things are not inherited. Thus the Communists, if they are logical from this point on, are driven to believe that those peoples whose environments have given them too little opportunity for mental and physical development must also, by reason of the inheritance of these effects through many past generations, have become stunted and inferior in their inborn capacities. Some leading Russian Communists have admitted to the author that they held to this version of the doctrine of the inferiority of economically underdeveloped peoples. Their dilemma concerning this point explains why, from the time the open attack on genetics was started in the USSR in 1936, the efforts that geneticists there had been making to refute the Nazi racial dogmas were all called off, and awkward but effective steps were taken to avoid being drawn into controversy on this crucial subject.

Since that time, however, after I had called their hand on this point,[4] other Soviet apologists, including the Soviet Academy of Sciences itself, have tried to dismiss the matter by resorting to the curious doctrine, not unknown elsewhere, that the laws of biological science stop short with man. Man, they proclaim, is a being of such a high order that only social laws apply to him. They do not attempt to reconcile this doctrine with such slogans as "Work and Bread," but it is not clear why, if man's alimentary system requires food, his reproductive system also does not work according to biological principles. This is a good illustration of the depths to which so-called scientific thinking has sunk when it is caught in the great spider-web.

Genetics is by no means the only branch of science that has been directly attacked by the Soviet totalitarianism, obviously on Stalin's orders. The germ theory of disease and the work of Koch have been derided by the Party-sponsored theory of disease of Speransky, which attributes most ailments to malnutrition of the nervous system. Important sectors of psychology, of astronomy, of quantum physics, and of statistical theory are among the other fields that have been openly assaulted. But it is evident that even the branches of science not subjected to a frontal attack must lose their vitality, their spirit of spontaneity and adventure, of free criticism and of objectivity, and the benefits from the advances made in the rest of the world, in the face of the restrictions, the interference, the insecurity, the terrorism engendered in scientific workers by the all-pervading despotism of Stalin. The conditions basic for continued scientific growth are gone. Although it will doubtless be possible to continue to milk the cow for awhile, the milk will get thinner and more meager, and will at last become positively poisonous.

If civilization does not have sound fundamental science to guide it, it is doomed eventually to decline through the action of numerous, slow-moving, insidious processes that are not evident to the superficial view. Illustrations can be drawn from mutation theory, from geochemistry, and from various other fields. But the question must also be asked, of what value, in any acceptable sense of the word value, would a civilization be in which men's minds were closed, so that they lived in an artificial, unreal world of dogma and illusion, from which they were not allowed to break out? Would it not be better to start afresh as pioneers, or even as savages, contending with the rigors of nature—yes, even with hunger, privation, and sudden death from wild beasts—until men could again rise by their own efforts, rather than to be plunged into the hopeless slavery and delusion of a totalitarian despotism? It would be shortsighted and selfish in the extreme if we should try to buy our own lives at the cost of the intellectual and cultural slavery of our children and descendants.

Let us hope, however, that we shall be confronted with no such decision. It may be that, instead, our long-oppressed brothers east of the Iron Curtain will manage to achieve a loosening of their bonds. We have no quarrel with them, but only the deepest sympathy. In the meantime, it is one of our obligations to exert ourselves to extend and make more effective our own intellectual, cultural, and material freedoms, thereby enriching and making more secure the heritage of our children, and increasing the moral and physical strength of all the people of this rapidly shrinking world. Remember, we should be very thankful that we have the right thus to declare that we ourselves do not yet have as much liberty, democracy, and opportunity, physical or intellectual, as our ideals demand. This right to think differently, to question, and to *express* our disagreements is the primary moral basis for the development of science and, indeed, for all that is valuable in the intellectual life of man.

[4] In an open letter to the Academy of Sciences of the USSR, published in *Science*, October 22, 1948.

For a New Academy[1]

Hans Nachtsheim

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MY ESTEEMED COLLEAGUE MULLER has taken up the questions here under discussion from a general point of view. Let me now say a few words about the German situation and, more specifically, about that in Berlin. I take it as a duty of the hour to speak in all frankness, although at the risk of having to say things that may not please this or that individual even in Western Germany.

German science is being threatened a second time by totalitarianism. German scientists cannot be spared the reproach that, before National Socialism came to power, they regarded it with too much indifference and unconcern. In wide circles the movement was not taken seriously and, when Hitler seized power on January 30, 1933, many of us still thought that the specter would soon disappear. Only as time went by were our eyes opened. But then it was too late, and the catastrophe took its course. Whoever dared to make a stand against the pseudo-science of the National Socialist race theory and its disastrous race policy was dismissed without his sacrifice winning an iota of success. We had let ourselves be gagged, and bound hand and foot. For the honor of German science, however, it may be stated that the majority of us stood in opposition, even though in silence. Of course, there were also those who, although in their hearts not National Socialists, sold their souls and outwardly, in words and writings, even in their scientific works, rendered repeated obeisance to the despotism. These opportunists have in my opinion always been the saddest figures in the Third Reich.

The Third Reich has been shattered, thanks to the help of a freedom-loving world. Thus there arises a great duty for the German scientist: the duty of being on guard and of not remaining indifferent and unconcerned a second time, when the issue arises of fighting a totalitarian system that recognizes no human rights. But are we German scientists really doing everything we can to fulfill this duty of ours to a world that seeks freedom?

Because of the small amount of space at my disposal I shall confine myself to a subject the discussion of which seems to me particularly urgent: the German Academy of Sciences in Berlin.

In July the East Berlin Academy celebrated its 250th anniversary. The Prussian Academy of Sciences, if it still existed, would have had the right and privilege, as the oldest academy of Germany, to celebrate the day on which it was founded by Leibnitz a quarter of a millennium ago. But does this East Berlin Academy of Sciences have the right to regard itself as successor to the Prussian Academy; is it still conducted in the spirit of Leibnitz? I venture to deny it.

In 1948 when, because of the pressure that was brought to bear upon me as director of an Institute of the Academy, I resigned from the institution, the members of the Academy assured me through their representative that they were all on my side in this matter, but neither their president nor the members were able to do anything about it: the decision rested solely with the Communist Administrative Director and his Soviet-German and Soviet-Russian superior authorities. The Academy is just as much under Soviet control as every university in the East Zone. When I expressed regret about this to a member of the Academy, it was felt necessary to reply that orders would not have to be followed in the case of elections for membership, and the name of a man was cited whose election had fallen through because of sharp resistance on the part of the members, in spite of the fact that his election had been called for repeatedly by the Soviet-German and Soviet-Russian authorities. At the next election a few months later, however, this man was taken into the Academy. And is it a sign of independence if, conversely, higher authorities are permitted to eliminate names from the ballot for purely political reasons?

Has anyone ever heard of a solemn protest by the Academy against the rape of science in Soviet Russia and against the kidnaping of internationally known scientists? I shall recall to you here only *one* name, that of Timoféeff-Ressovsky. For many years Timoféeff-Ressovsky, one of the outstanding geneticists of our time, carried on preeminent work at the Kaiser-Wilhelm Institute in Buch, now an institute of the German Academy, until he was abducted in 1945 by the Russians. Two years later his wife, likewise an internationally known geneticist, was abducted also. Since then we have not learned anything certain about either of them. We suspect that they are lost to science, in a camp in Siberia. Would it not be a lofty task for the Academy to use its good offices with the Soviets in order to regain the Timoféeff's for science? Instead, the Academy, through its president, sends to the world's greatest tyrant and greatest enemy of cultural freedom, on the occasion of his seventieth birthday, an epistle which, in my opinion, represents the depth of debasement of a German scientist. This telegram deserves to be read word for word.

On his seventieth birthday the German Academy of

[1] Based on an address delivered at the panel on "Science and Totalitarianism" of the Congress for Cultural Freedom, Berlin, Germany, June 27, 1950.

Sciences in Berlin sends its respectful congratulations to that great statesman, of unique authority, the friend of all peace-loving and freedom-loving peoples, the thinker who has penetrated with the clarity of a seer to the deepest fundamentals of historical and social life, and who has become the teacher of his people and of all progressive humanity. May the hoped-for new order of the world and the peace that brings reconciliation of peoples lead to those goals which his power and wisdom have pointed out. May Germany's role in this, following Stalin's word, be that of a guardian of the new world peace, based on that forever sealed friendship with the peoples of the Soviet which has again been confirmed by him.

In the Third Reich, also, telegrams that made us blush with shame were sent by scientists to Hitler. But, to my knowledge, nobody ever reached such a degree of humiliation as in this telegram to Stalin. Moreover, in Hitler's time we were confronted with a tyrant of our own country, whereas here we are dealing with a foreign one. This telegram of the president of the Academy is said to have been sent off without the knowledge of its members. Intramurally it occasioned strong protests. But with these intramural protests the matter rested. I do not know of any member at all who has taken any action in consequence of it.

Recently there has been a flood of new members into the Academy, in which not only the East Zone but also West Berlin and Western Germany have been involved. I have had no information as to whether any scientist in Western Germany has gratefully declined this honor. I only know that one of those newly elected, the editor of a scientific journal in Western Germany, sacrificed a page of his journal in order to announce this honor to his readers in the middle of the page. The election to membership, be it noted, followed weeks after the telegram to Stalin. Political blindness strikes one. One imagines oneself to be working purely scientifically and does not notice that he has become enmeshed in the Communist propaganda machine, so as now to be working for Communism.

Of course, I am not of the opinion that one should desert the venerable old Prussian Academy of Sciences and let it perish. But the Academy in East Berlin, so long as it is under Soviet domination, can never be a stronghold of free science. Why did the Academy not go into exile long ago? Why has a restorative organization not been set up in Western Germany? Or why, to propose another method, do the Academies in Western Germany not offer sanctuary to those members of the old Prussian Academy who are not stained with Nazism? Complete indolence in these matters is, in any case, impossible and intolerable. The Berlin Academy has not yet found its "Seven of Göttingen" for an act of liberation.

In view of this state of affairs it cannot come as a surprise that the British Royal Society, the Académie Française, and the University of Stockholm, to name only a few, declined to participate in the festivities of the so-called 250th anniversary celebrations of the East Berlin Academy. Perhaps this fact will open the eyes of some of its members.

More courage of their convictions among German professors! That is the demand we make in deep concern for the future of Germany. The Weimar Republic perished because of the indolence of its citizens, because of the tolerance of its intellectuals toward the intolerance of National Socialism. May we German scientists bestir ourselves that the cultural freedom which we in the West have regained, thanks to the help of our Western Allies, will again be shared by the East and will not, through tolerance toward the intolerance of Communism, be lost again even in the West.

(Excerpt from German newspaper of first week in July, to show results already accrued from Nachtsheim's speech. This speech, like his previous actions, was especially courageous in view of the fact that over 4,000 persons who were persona non grata *to the Soviet authorities had already been abducted by Soviet agents from the Western Sector of Berlin.*—H. J. Muller.)

FURTHER WITHDRAWALS FROM THE EASTERN ACADEMY

PROFESSORS DISSOCIATE THEMSELVES—A CONSEQUENCE OF THE CONGRESS FOR CULTURAL FREEDOM

D. T. Berlin. Four eminent scholars of Berlin have followed the example of Professor Alfred Weber, of Heidelberg, who withdrew from the Academy of Sciences of the Soviet Zone shortly after he found out about a telegram which this body had sent to Stalin and had hitherto kept secret. These professors of the Free University, Friedrich Meinecke (modern history), Wolfgang Heubner (dermatology), Walter Lohlein (ophthalmology), and Richard Thrunwald (folklore), have issued an explanation of their withdrawal from the Eastern Academy, which states the following: "All of us have only accidentally, after several months' delay, learned of the unworthy act of the president of the Academy, in arbitrarily sending off the telegram for Stalin's 70th birthday, without consultation with the Plenum. As this shameful event has now been publicized by Mr. Nachtsheim before the international forum of the Congress for Cultural Freedom, we are concerned lest our membership in the Academy—even though it has for a long time been only a formal one—be widely misconstrued. In order that no one may think that we underrate occurrences and developments which are inconsistent with the spirit of freedom, we are now dissolving even this formal connection."

Technical Papers

Experimental Confirmation of a Previously Reported Unusual Finding in the Blood of Cyclotron Workers[1]

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In an earlier paper (*1*) the authors reported the occurrence of lymphocytes with bilobed or double nuclei in the blood of personnel associated with the 130-inch cyclotron at the University of Rochester. The cells were noted after exposures which were believed to be well below the accepted tolerance levels. The relationship between the increased incidence of lymphocytes with bilobed nuclei and exposure to small amounts of radiation from the cyclotron has now been confirmed experimentally in this laboratory, and considerable additional information has been obtained relative to the occurrence of the cells in cyclotron personnel. Because of the interest which has been expressed in this finding by individuals associated with cyclotrons in other institutions, the experimental results are presented briefly. Analysis of the findings in personnel is being completed and will be the subject of a subsequent paper.

Hematological studies have been made on 3 healthy mongrel dogs repeatedly stationed in relatively protected positions outside the cyclotron building for short periods while the cyclotron was running. These positions, which were roughly 50 ft from the target, were about at the level of the tank and were well outside the neutron beam. Each dog had daily blood studies for approximately 2 months prior to exposure. Several times during the control period the dogs were fastened by a chain in their assigned positions outside the cyclotron building while the cyclotron was not running. No significant changes in the blood picture occurred during the entire control period.

On August 22, October 4, and December 12, 1949, the dogs were fastened in their assigned positions for 30 min while the cyclotron was operating. The dogs' positions were changed with each exposure so that each dog was exposed once in each of the 3 positions. Operating conditions were maintained as nearly constant as possible; however, there were certain inherent variations in the cyclotron over the period of time under consideration, and it is probable that there were definite differences in magnitude of the 3 exposures. Nuclear track plates[2] were taped to the collars of the dogs during the first 2 exposures. They yielded relatively little quantitative information regarding the amount of radiation received, and, in fact, measurement of doses was not possible in any of the exposures. It is felt that exposures were relatively slight, probably below the currently accepted tolerance levels. This assumption is based on (1) earlier studies of the relative intensity of radiation at various points around the cyclotron building, and (2) the fact that the usual hematological signs of overexposure to ionizing radiation were minimal or absent.

Lymphocytes with bilobed nuclei were detected by microscopic examination (oil immersion) of cover-slip blood smears. Good cover-slip smears were selected and stained with peroxidase stain. Each smear was examined from edge to edge, and the number of white blood cells in all good areas was recorded. (As a rule, several thousand white cells are distributed throughout the good areas of a single well-pulled smear of dog blood.) The position of each abnormal cell was recorded in terms of the setting of the graduated mechanical microscope stage, and the classification of each of the cells was subsequently checked by the entire group concerned with the hematological examinations. Photomicrographs were made of many of the cells. The frequency of occurrence of blood smears containing one or more lymphocytes with a bilobed nucleus has arbitrarily been taken as an index of the incidence of these unusual cells.

Blood samples were obtained both morning and afternoon during the first post-exposure week, and once daily thereafter until the experiment tapered off with less frequent counts subsequent to the sixth week following the second exposure and the third week following the last exposure (Table 1). The complete experiment continued over a period of approximately one year. A total of approximately 180 smears was examined during the control period, and a like number during each first post-exposure week. Thereafter approximately 30 smears were examined each week. In all, approximately 3.3×10^6 white blood cells were examined during the experiment.

Examples of lymphocytes with bilobed nuclei are shown in Fig. 1. The cells are slightly larger than typical large lymphocytes, show typical lymphocyte staining with Wright's stain, and are peroxidase-negative. The cytoplasm is clear, light blue, and sometimes appears slightly "clumpy" in peroxidase-stained smears.

The data relative to the occurrence of lymphocytes with bilobed nuclei are presented in Table 1. There was some individual variation in the occurrence of the cells in the 3 dogs; however, these differences do

[1] This paper is based on work performed under contract with the U. S. Atomic Energy Commission and the Office of Naval Research at The University of Rochester Atomic Energy Project and 130-inch Cyclotron Laboratory, Rochester, N. Y.

[2] Kindly made available and interpreted by Herbert Mermagen.

TABLE 1

INCIDENCE OF LYMPHOCYTES WITH BILOBED NUCLEI IN DOGS EXPOSED TO RADIATION FROM 130-INCH CYCLOTRON

	Control period†	Post-exposure period				
		1st week	2nd week	3rd and 4th weeks	5th and 6th weeks	After 6th week
First Exposure						
Percentage of positive smears*	0.6	22.0‡	37.0‡	11.0	2.0	.....
White blood cells examined	372,000	400,000	70,000	194,000	253,000	.....
Smears examined	180	148	30	54	60	.....
Positive smears	1	32	11	6	1	.....
Second Exposure						
Percentage of positive smears		16.0	13.0	6.0	3.0	0
White blood cells examined		430,000	114,000	206,000	208,000	112,000
Smears examined		142	30	62	60	36
Positive smears		22	4	4	2	0
Third Exposure						
Percentage of positive smears		19.0	6.0	11.0	.....	3.0 .....
White blood cells examined		520,000	90,000	18§	.....	96‖ .....
Smears examined		161	30	53,000	.....	288,000 .....
Positive smears		39	2	2	.....	3 .....

* Smears containing one or more lymphocytes with bilobed nuclei are considered "positive."
† Control counts represent a period of approximately 2 months before first exposure.
‡ Associated with a slight depression in the total white blood cell count.
§ Counts were done only 3 days during the third week.
‖ Based on counts done once a week from the fifth through the seventeenth post-exposure weeks.

not significantly alter the interpretation of results. Such differences as did occur appeared to be related to individual variations among the 3 dogs and not to differences in doses at the 3 positions.

The experimental results clearly demonstrate a marked increase in the occurrence of lymphocytes with bilobed nuclei following exposure to radiation from the cyclotron. The incidence was maximal during an approximately 2-week period following exposure.

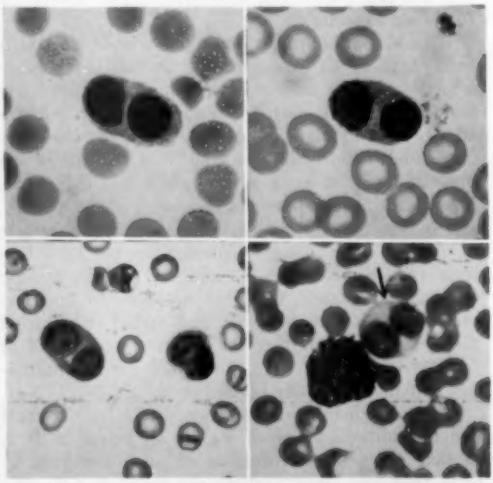

FIG. 1. Left to right: Lymphocyte with bilobed nucleus (peroxidase stain). Other cells are red blood cells. Lymphocyte with bilobed nucleus (Wright's stain. This cell contains several azurophile granules. One lymphocyte with bilobed nucleus, one normal lymphocyte (peroxidase). Normal granulocyte showing characteristic dark peroxidase positive granules and one lymphocyte with bilobed nucleus (arrow).

Thereafter the frequency of positive smears decreased. The highest incidence occurred after the first exposure and was accompanied by a slight depression of the total white blood cell count as determined by routine diluting and counting procedures. The total white blood cell count was not depressed following the second and third exposures. This greater response after the first exposure probably reflects a higher dose. As mentioned above, however, it has not been possible to make quantitative physical estimations of the amount or types of radiation delivered in any of the exposures. It is, of course, also impossible at this time to state which component or components of the radiation are chiefly responsible for producing the "bilobed-nucleus response" in lymphocytes, although studies in this direction are currently being carried out in this laboratory. In our experience, however, the response has been less prominent in persons working with the 26-inch cyclotron and with radioactive isotopes than in personnel associated with the 130-inch cyclotron. R. E. Carter has observed this type of cell in certain personnel at Los Alamos (*2*), and Ethel Browning reports having occasionally seen the cells in the blood of "luminizers and operatives using x-rays and the radium bomb for industrial purposes" (*3*). It should be pointed out, however, that the cells are not an absolute indication of radiation effects. We have observed this type of cell in certain infectious diseases of childhood, as well as in infectious mononucleosis, lymphatic leukemia, etc. As in the case of any hematological finding, interpretation requires careful evaluation of the general condition of the individual.

Perhaps the most significant aspect of the findings reported here relates to the observation that the cells occur following extremely small exposures that do not produce changes detectable by routine hematological

examinations. For this reason studies of the occurrence of lymphocytes with bilobed nuclei appear to be an unusually promising means of identifying potentially harmful operations *before* gross overexposure can occur.

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Virus Strains of Identical Phenotype but Different Genotype

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Delbruck and Bailey (*1*) noticed an anomaly in the lysate of bacteria which was obtained by mixedly infecting the B strain of coli with the bacterial viruses T2 and T4. Subsequently, Luria (*2*) found this anomaly to be even more pronounced when he repeated Delbruck's experiment—using, however, virus T2 that had been exposed to ultraviolet irradiation.

When we undertook experiments in an attempt to understand this anomaly, we were led to the following result: If we infect a culture of the B strain of coli mixedly with the bacterial viruses T2 and T4 and incubate to permit lysis of the bacteria, there are present in the lysate 3 easily distinguishable types of bacterial viruses. Two of these, as expected, behave like the original parent strains T2 and T4, i.e., one of them behaves like T2 inasmuch as it is unable to attack the mutant strain B/2 (which is resistant to T2) but is able to grow in the mutant strain B/4 (which is sensitive to T2); the other behaves like T4, being unable to attack B/4 (which is resistant to T4) but is able to grow in B/2 (which is sensitive to T4). The third type of virus present is phenotypically like T4 inasmuch as it is capable of multiplying in the strain B/2 (which is sensitive to T4), but it is genotypically like T2 inasmuch as, after one passage in the strain B/2, it is no longer capable of growing in it but is capable of growing in the strain B/4 (which is sensitive to T2).

The presence of this third type of virus, which may be called "latent T2," can be demonstrated in the following manner: We add to a culture of the B strain of coli viruses T2 and T4 in ratios corresponding to 10 T2 and 10 T4 virus particles per bacterium, incubate to permit lysis of the bacteria, and then filter the lysate.

If we plate a sample of this lysate on agar that is inoculated with the strain B/4 (which is sensitive to T2 but resistant to T4), those virus particles contained in the lysate which have the phenotype T2 will show up as plaques on these plates. T4 virus particles will not give plaques on this plate because B/4 is resistant to T4. The number of plaques is thus a measure of the number of T2 particles in the lysate.

Using a sample of the lysate, we determine in this manner the number of plaques obtained on an agar plate inoculated with the strain B/4. When we repeat this experiment—with the difference that before plating on the B/4 plate we add to the sample of our lysate a certain quantity of the strain B/2, allow 5 min for absorption, dilute with broth, and incubate for 1 hr to permit lysis of the bacteria—then we obtain a ten to twenty-five times larger number of plaques on the B/4 plate.

This phenomenon appears to show that there is present in our lysate a virus (the "latent T2") which is capable of multiplying in B/2 and subsequently forming plaques on B/4. In order to account for our observation, the concentration of the "latent T2" in the lysate would have to be about 10% of the concentration of T2. We were not able to obtain, after one passage in B/2, any appreciable further growth in B/2 of our hypothetical "latent T2." Before drawing the conclusion that the presence of a "latent T2" is in fact responsible for our phenomenon, it is necessary to exclude alternative explanations.

As an alternative explanation of our observation, it appeared a priori conceivable that our lysate contains aggregates of virus particles formed by a T2 and a T4 particle that stick together. Such aggregates might then perhaps be capable of entering into a bacterium of the strain B/2 (by virtue of their T4 component) and, once inside, both virus particles T2 and T4 might then be able to multiply, and thus to produce the observed phenomenon. We were able to rule out this possibility, however, by performing the following experiment.

We add to a sample of our lysate a certain quantity of B/2, using an excess of B/2 so that independent infection of one bacterium by more than one virus particle can be neglected. We then allow 5 min for absorption and plate on an agar plate that has been inoculated with both B/2 and B/4. If there are present any B/2 bacteria into which has entered an aggregate of virus particles composed of T2 and T4, and in which both viruses will grow, then a certain number of clear plaques centering around such bacteria (which yield both T2 and T4) should develop on the agar plate. We were not able to find any such clear plaques, however, and found only turbid plaques (in which either the B/2 is lysed by T4 or the B/4 is lysed by T2). This rules out the alternative explanation of our phenomenon.

We ascertained that our phenomenon is produced under conditions in which we use an excess of B/2, so that independent infection of one bacterium by more than one virus particle can be neglected. We also ascertained that our phenomenon is not produced if, in place of our lysate, we use a mixture of T2 and T4.

We are thus led to conclude that the phenomenon described is due to virus particles that have the phe-

notype of T4, but the genotype of T2. The properties of this "latent T2" virus would seem to merit investigation.

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Paper Disk Columns in Glass Chromatographic Tubes

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The use of filter paper in selective adsorption experiments, probably first described by Schönbein (*1*), has become a powerful tool in research, especially since the studies of Consden, Gordon, and Martin (*2*). Although in most modifications of paper chromatography only very small amounts of material can be resolved, Mitchell and his colleagues must be credited with preparative applications; recently these authors carried out successful enzyme separations in their chromatopile (*3, 4*).

Up to the present time no use has been made of the classical Tswett glass tube packed with filter paper disks, for the following reason. Although a homogeneous, satisfactory column may readily be prepared from a powder, on the contrary, channeling and gross irregularities in the flow of solutions become manifest when paper disks are stamped into a cylindrical tube.

It was found that this difficulty can be overcome if the size of the disks is adapted to the average diameter of a good cylindrical tube by cutting the paper with unusually high precision. For this purpose a stainless steel die and a corresponding punch must be available by means of which a supply of precision-cut paper can be obtained. About 15 paper disks were cut simultaneously with our die when Eaton and Dikeman No. 615 filter paper was used. The same cutting device can, of course, be used to prepare paper for several glass tubes,[2] represented in Fig. 1, which possess similar diameters.

For example, the diameters of 2 tubes (measured in different directions and at various levels inside the tube) varied, respectively, between 1.716–1.730 in. (4.359–4.394 cm) and 1.730–1.736 in. (4.394–4.409 cm). The diameter of the die was 1.73064 in. (4.396 cm), and that of the punch was 1.7299 in. (4.3938 cm). If the average diameter of the glass tube was, for example, 1.68 in. (4.27 cm), the even flow of the solvent was markedly distorted.

In order to pack the chromatographic tube, the procedure shown in Fig. 2, upper part, is recommended. Each portion (20 paper disks, for example) is somewhat loosened up by ruffling the edges with the thumb, inserted a few centimeters deep into the horizontal glass tube and pushed in further with a smooth, cylindrical pestle (diameter, about 1 mm smaller than that of the tube), while the other hand is holding firm both parts of the glass tube (*cf.* Fig. 1) and exerting pressure. When the paper has reached its proper location, considerable pressure is exerted with the pestle for a moment while it is rotated by about a quarter turn. A column 20–25 cm high can be built up in about 15 min.

Fig. 1.

When suction of an ordinary water aspirator is applied, either aqueous or nonaqueous solutions flow evenly through a paper column which has been packed in the described way. The duration of the chromatographic experiment will then not exceed that required for a powder column of similar dimensions.

Fig. 2. Packing and removing a filter disk column. Left hand holds the ground glass section (*cf.* Fig. 1); dark area on left indicates position of the rubber stopper by means of which the tube will be attached to a suction flask.

Because of the tight contact between glass and paper, only short and broad columns can be extruded with a pestle or removed by means of a long-stem corkscrew; in general, however, the procedure as indicated in Fig. 2 (lower part) is recommended. About 1-cm sections of the column are rapidly taken out by using a sharply pointed and slightly bent steel pick which has a tempered end about 5 cm long.

The proposed technique shares the advantages of ordinary powder column experiments—i.e. those of a reasonably closed system—but has the disadvantage that the basic equipment requires the services of a precision mechanical shop.[3]

[1] Contribution No. 1436.

[2] The model used is the same as proposed earlier (*5*) and is manufactured by the Scientific Glass Apparatus Co., Bloomfield, N. J.

[3] We are indebted to J. H. Sturdivant for the facilities of the workshops under his direction.

In the field of the enzymes, as was recently shown in collaboration with Rohdewald (*6*), a number of enzymes can be located on powder columns by painting a longitudinal streak with a brush carrying the solution of the corresponding substrate; after a brief incubation period a second brush applies a color reagent for the enzymatic cleavage product and thus indicates the borderlines of the zone. In any system of paper disks such operations will be carried out, of course, in the manner of spot tests as proposed earlier by Feigl and others for certain enzymes (*7*).

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Grasshopper Transmission of Three Plant Viruses[1, 2]

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The tobacco mosaic virus, potato virus X (latent potato virus), and tobacco ringspot virus have been studied intensively, but heretofore the satisfactory transmission of these viruses by insects has not been clearly shown. Investigations carried out in the greenhouse show that the differential grasshopper, *Melanoplus differentialis* (Thos.), can transmit these viruses from tobacco to tobacco.

Allard (*1, 2*) reported transmission of a virus thought to be tobacco mosaic, from tobacco to tobacco with the aphids *Myzus persicae* (Sulz.) and *Macrosiphum tabaci* Perg. Hoggan (*3*) indicated that *M. persicae* does not transmit the tobacco mosaic virus and suggested that Allard was working with the cucumber mosaic virus, which *M. persicae* does transmit. Hoggan (*4–6*) demonstrated that *M. persicae*, *M. pseudosolani* (Theob.), and *Macrosiphum solanifolii* (Ashm.) do not transmit the tobacco mosaic virus from tobacco to tobacco, although they will transmit it from tomato to tobacco and other solanaceous hosts. Gigante (*7*) has reported transmission of the tobacco mosaic virus by *Macrosiphum gei* Koch (= *M. solanifolii*) from tobacco to tobacco, tomato, pepper, and eggplant. No insect vector has been discovered for potato virus X and tobacco ringspot virus.

Single adult grasshoppers were used for all tests. The insects were reared on young corn plants and starved 4–6 hr before feeding on the virus source. Infected tobacco (*Nicotiana tabacum* L.) served as the source for the 3 viruses. A 1–2-minute feeding on the virus source was allowed for transmission tests made immediately after the feeding. For the 2- and 4-hr waiting periods[3] a 15-min feeding time was allowed, and for the 12- and 24-hr tests, a 30-min feeding time. Each insect was transferred by hand. It was caught by a hind leg with the thumb and forefinger and placed upon a leaf of the test plant. This method of handling was used to prevent transfer of the viruses by hand. Precautions were also taken to avoid infection from other sources. Unless otherwise stated, each grasshopper was moved to a new location on the test plant after feeding a few bites or making a hole 1–6 mm in diameter in the leaf. Except as noted, each grasshopper was allowed only 2 feedings at different locations on the test plant in the tests made immediately after feeding and after the 2- and 4-hr waiting periods. Those individuals tested after a 12-hr waiting period were allowed to feed in approximately 10 different locations on the test plant. Sixty-four per cent of the insects tested after a 24-hr waiting period were allowed to feed in 2 locations, and 36% at 10 locations.

A hybrid plant, *N. tabacum* × *N. glutinosa* L., which develops local lesions at the point of infection, or necrotic streaks if the virus is introduced into the veins, was used for the transmission tests with the tobacco mosaic virus. Single grasshoppers were tested for infectivity on individual plants, and the local lesions which developed were counted. Seventy-five grasshoppers were tested immediately after feeding on the virus source. Twenty-eight local lesions developed on 22 plants, which indicates that 22 (29.3%) of the 75 insects tested transmitted the virus. Seven (46.6%) of 15 insects tested after the 2-hr waiting period transmitted the virus. After the 4-hr waiting period, 10 (29.4%) of the 34 grasshoppers tested brought about the production of 12 local lesions. Only 2 (9.5%) of the 21 individuals tested after the 12-hr waiting period transmitted the virus. Tests with 58 insects following the 24-hr waiting periods resulted in no transmission.

In another experiment, individual grasshoppers were allowed to feed for a series of 18 times, one on each half of 9 leaves, after feeding on infected plants. Forty (83.3%) of 48 insects tested transmitted the virus, producing a total of 93 local lesions. The greatest

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[2] This paper is based upon work done for the Biological Department Chemical Corps, Camp Detrick, Frederick, Md., under Contract No. W-18-064-CM-226 with the University of Nebraska.

[3] "Waiting period" refers to the time between the feedings on the virus source and the test plants, during which time no food was consumed.

number of lesions occurred adjacent to feeding areas numbered from one to six. The occurrence of local lesions did not follow a definite pattern. In some cases they were consecutive, and in others as many as 16 feeding areas occurred between those producing lesions.

Another experiment consisted of feeding single grasshoppers on the virus source for 5 min and then immediately transferring them to individual hybrid plants to feed at will from 4:00 P.M. until 9:00 A.M. the following day. The amount of feeding ranged from single bites to almost complete consumption of the leaves. Twenty grasshoppers were tested, and 50% transmitted the virus, producing a total of 15 local lesions.

Local infection of the hybrid plants was not always adjacent to a feeding area. Approximately 3% of the total local lesions that developed in all tests occurred elsewhere. This might be due to transfer of the virus by the feet of the insects; however, grasshoppers do scar or make depressions in the leaf tissue with their mandibles while in search of a suitable feeding site. The latter seems to be the more plausible explanation. The feeding on a vein, or the development of a local lesion adjacent to one of the larger veins, brought about systemic infection of the hybrid plants in several instances.

In one experiment, single grasshoppers were transferred to individual healthy tobacco (*N. tabacum*) plants immediately after feeding on the virus source and were allowed to feed approximately 10 times in different locations. Fifty-seven (54.8%) of the 104 insects tested transmitted the tobacco mosaic virus, causing systemic infection of the plants. After a waiting period of 2 hr, 9 (45%) of the 20 insects tested transmitted the virus; and 9 (42.9%) of the 21 individuals tested after a 4-hr waiting period brought about systemic infection of the test plants.

Tobacco was also used as the test plant for the transmission of potato virus X (potato ringspot type) and tobacco ringspot virus. Single grasshoppers were transferred to individual healthy plants immediately after feeding on the virus source and were allowed to feed 6–8 times in different locations. The insects were disturbed between feedings, making the leaf perforations appear as isolated holes. Local infection appeared within 5–6 days. For the potato virus X, 18 (18%) of the 100 grasshoppers tested transmitted the virus to tobacco plants, which developed local lesions and later became systemically infected. Four of the initial infections did not occur adjacent to apparent feeding areas. With tobacco ringspot virus only 6 (6%) of the 100 insects tested transmitted the virus. Initial infection, with one exception, occurred adjacent to known feeding areas. In a majority of the cases, with both viruses, initial infection developed adjacent to feeding areas near one of the larger veins.

These experiments show that tobacco mosaic virus, potato virus X, and tobacco ringspot virus can be transmitted with the differential grasshopper under conditions as described above. This is possibly a simple mechanical transmission and nonspecific. The importance of grasshoppers transmitting these viruses under field conditions has yet to be investigated. A more detailed account of the work relating to this problem will be published elsewhere.

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Selective Damage to Fibroblasts by Desoxycorticosterone in Cultures of Mixed Tissues

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The remarkable selectivity which adrenocortical steroids appear to show for mesenchymal tissue, as demonstrated in pathological and clinical studies (*1*), raises the question as to whether this represents a direct effect on the cells of connective tissues. In rheumatic fever the connective tissues of the heart are profoundly altered by necrosis and fibrosis, whereas the changes in endothelium and in muscular activity appear to be secondary. Desoxycorticosterone (DOC) induces similar changes, but not in hypophysectomized animals (*2*). In this study it is proposed to explore the possibility of comparable *in vitro* effects of adrenal steroids on heart tissues, particularly with a view toward establishing whether there is one target tissue.

Hearts were removed from newborn line C white mice, cut into fragments about a millimeter across, and cemented into roller tubes with chicken plasma. Such fragments continued to pulsate in the cultures and at the same time provided outgrowths of two kinds of cells. The most abundant cells were of the fibroblast type, elongate bipolar cells arranged in radiating strands or in irregular networks (Fig. 1, right). The cells believed to be endothelium grew as continuous sheets of polygonal cells (Fig. 1, left). The initial growth was in nutrient solution: balanced saline (Gey's) + serum + embryo extract. The nutrient was then replaced by balanced saline containing the experimental agent. Desoxycorticosterone was used in doses sufficient to produce cytological changes in 6–24 hr. At this time the experimental solution was replaced with nutrient solution. It was thus possible to follow the relative rapidity of response of the two types of

[1] I am indebted to Clifford Grobstein, of the National Cancer Institute, for mice used in these experiments. This investigation was financed from an institutional grant to the Helen L. and Mary E. Warwick Memorial for Cancer and Allied Diseases by the American Cancer Society, Inc.

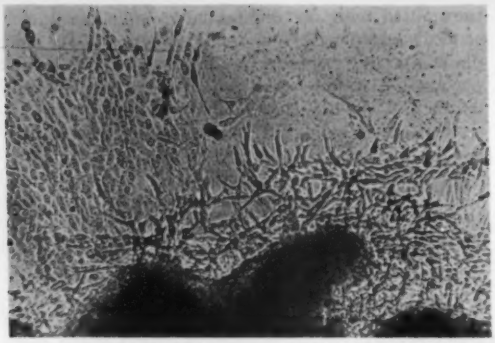

FIG. 1. Two-day growth from a heart fragment. Endothelium to the left, fibroblasts right (approx. × 100).

tissue, and later to compare the recovery as revealed by new growth from the explant.

For most of these studies DOC purchased from Delta Chemical Works was used. The results have been checked with DOC contributed by Ernst Oppenheimer, of Ciba Pharmaceutical Company. The two products differ in physical and biological behavior. The Delta sample, received as a lump of slightly yellow material, when dispersed in balanced saline at 1:100 from a 1% acetone solution, formed an opaque suspension which later coated the side of the tube. The Ciba material was white and finely divided. The 1% acetone stock solution diluted 1:10 in balanced saline was faintly opalescent and later became milky and precipitated loose crystals. DOC acetate (Delta), cortisone (Merck), and cortisone acetate (Merck) were also tested.

In the 2–4 days following planting, 12 (2%) heart fragments had grown only endothelium, 144 (25%) had grown both endothelium and fibroblasts, and 414 (73%) had produced only fibroblasts. Upon exposure to 0.02 mg/ml of Delta DOC or 0.1 mg/ml of Ciba DOC, cytological changes became visible by 16 hr. At 0.03 mg/ml of Delta DOC or 0.2 mg/ml of Ciba DOC, changes were seen at 6–7 hr. Some cells became rounded, as is characteristic of the response to most noxious agents, but the typical DOC-induced alteration is best described as a moderate shriveling. The fibroblast cell membrane lost its smooth contours and became angular; the cytoplasm became granular, and the cells narrowed to a half or a third their usual width, although the filamentous processes remained extended. Endothelium similarly became granular, and the cells sometimes shrank, separating the membrane into isolated or grouped cells.

At these doses the original outgrowth usually never recovered. In one experiment, however, exposure to 0.1 mg/ml of Ciba DOC for 25 hr did not damage the endothelium. The rounded and shriveled fibroblasts (Fig. 2) never recovered, but the endothelium which lay between the fibroblasts and the DOC solution merely became granular and upon return to nutrient resumed normal morphology and growth. In the more severely damaged cultures about a fourth of the DOC-treated explants produced new fringes of cells. In contrast to the initial low percentage of endothelial growth, the regenerated tissue comprised 34 (57%) explants with endothelium alone, 14 (24%) with endothelium and fibroblasts, and 11 (19%) with fibroblasts alone. These same explants before treatment included only 1 (2%) with pure endothelial growth, 28 (47%)

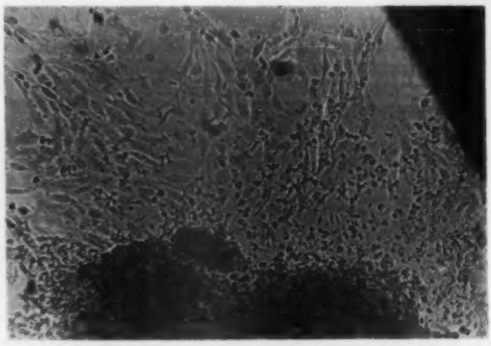

FIG. 2. Original outgrowth of cells after 24-hr exposure to 0.1 mg/ml DOC (Ciba) followed by 2-day recovery in nutrient solution. The endothelial cells (left) are expanded and transparent. The fibroblasts (right) are rounded or shriveled and densely granular. The endothelium is growing, but the fibroblasts have remained unchanged for 2 days (approx. × 100).

with endothelium and fibroblasts, and 30 (51%) with fibroblasts alone. All those that on recovery produced pure endothelium had some fibroblasts originally. Thus, fibroblasts were eliminated from 34 explants, whereas endothelium was lost from only 3. Chi-square analysis shows the frequencies of the tissues in the treated cultures to be significantly different from the frequencies in the same explants before treatment, and from the frequencies in all untreated cultures. In untreated cultures the distribution of endothelium and fibroblasts did not change during the 2–3 weeks the cultures were followed, but the area occupied by fibroblasts increased more rapidly than did the endothelial area.

Cortisone at 0.05–0.15 mg/ml added to DOC accelerated and increased the visible cytological alterations (which included more rounding of cells than caused by DOC alone), but did not interfere with the favoring of endothelium in explants which survived. When 10% of the saline was replaced by serum, DOC (Delta) exerted no visible effects in doses up to 0.03 mg/ml. DOC acetate at 0.031 mg/ml, or cortisone acetate at 0.026 mg/ml (both in excess of saturation), had no effect on the 30 explants tested with each.

In most DOC cultures where selective damage to fibroblasts was obtained, the acetone used as the initial solvent for DOC did not exceed 0.2%. Where high doses of steroids were tested, the concentration of acetone sometimes reached 1%. Acetone or ethanol alone at 1% had no perceptible effect on the cells. Increasing the dosage slowly over a period of hours until 2.3%

acetone or 2.6% ethanol had been added caused both the fibroblasts and endothelial cells to round up.

The pulsation of the heart fragments was diminished or stopped by concentrations of DOC which damaged the cells, and not by DOCA at saturation (*3*). The pulse returned when the DOC was removed.

Isolated heart tissues in culture are damaged by DOC but not by its acetate. This may result merely from the low solubility of the acetate, or may indicate that these tissues lack the enzymes by which the intact body is able to utilize DOCA. The DOC effect, elimination of fibroblasts from tissue cultures, is quite the reverse of the trend in untreated cultures. Usually the fibroblast is the hardiest of cells in culture and outgrows most other cell types, although poisons specific for mesenchymal cells are known (*4*). Indifferent poisons such as ethanol or acetone affect endothelium and fibroblasts equally, causing rounding of both at threshold doses. The alkaloid ryanodine, by way of contrast, causes the endothelial cells to round up while the fibroblasts remain extended (unpublished data). Nonadrenal steroids must be tested in cultures, however, before any correlation with systemic effects of adrenocortical hormones can proceed very far.

It is tempting to draw a complete parallel between the selective damage to fibroblasts in culture by DOC and the necrosis of vascular and cardiac fibrous tissue in rheumatic diseases. Selye and co-workers have induced periarteritis (*5*) and Aschoff bodies (*6*) in rats by injection of DOCA. Moreover, the dose (1 mg/day) used by Selye to produce periarteritis nodosa in sensitized rats is close to the levels employed in tissue culture on a mg/kg basis.

There are obstacles to this line of reasoning, however. Although cortisone induces an atrophy of the dermis when applied directly to the skin (*7*) and inhibits formation of granulation tissue (*8*), early arthritic lesions induced in rats by DOC acetate are highly cellular (*5*). Also, granulation tissue is well populated with large, many-branched fibroblasts in rats receiving DOCA, whereas rats treated with sex hormones produce a granulation tissue sparse in both cells and fibers, and the cells are thin and small (*9*). The DOC-treated fibroblasts in culture cease to proliferate and shrink to a narrow or even threadlike configuration. The similarity of these two pictures suggests that the adrenal hormone effects reported here are nonspecific steroid influences. There remains some reason for doubting, however, that such a nonspecific steroid effect occurs in the absence of adrenal hormones, inasmuch as the local inhibition of hair growth induced by estrone is dependent upon the adrenals (*10*).

At least, the picture of adrenal cortical influence is somewhat clarified by a demonstration of a direct selective effect of adrenal steroids upon fibroblasts isolated from all systemic influences. The lethal effects observed in tissue culture are probably in excess of inhibitory phenomena within the body, but high concentrations of DOC were employed in order to get as close as possible to an all-or-none selection of the resistant tissue. Possibly at lower doses more subtle effects reflecting the physiological specificities and antagonisms of adrenal steroids will be detected in tissue cultures. Also, fixed cultures should be employed in an attempt to follow cytological changes such as those reported by Schneebeli (*11*).

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A Simple Presumptive Test for Toxigenicity of *Corynebacteria*[1]

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Laboratory methods of determining toxin production among the *Corynebacteria* depend upon the use of animals. As a result, many laboratories report on morphology of such cultures only. Previous work has indicated that chick embryos are susceptible to pure toxin (*1*), and to whole broth cultures (*2, 3*). However, only 3 strains of *Coryn. diphtheriae* and one of *Coryn. hoffmannii* were used. It occurred to the author that a method being used in an investigation of this group of organisms, now in progress, was sufficiently sensitive and reliable to warrant a brief report. The simplicity of the method, and ready availability of the test animal, should make it practical even in small laboratories.

Essentially, the procedure consists of inoculating a light suspension of the culture on the chorio-allantoic membrane of 9–10-day-old embryonated hen's eggs. The culture is purified by plating, and a loopful of growth from a 24-hr-old blood agar plate is suspended in broth and washed twice, centrifuged and resuspended in fresh broth. The sediment from the last centrifugation is emulsified in a few drops of broth, and a loopful is transferred to about 10 ml of broth. The turbidity should be just visible in a bright light. Such suspensions give reproducible results with the same culture and from one culture to another.

Eggs at the 9–10-day stage of incubation are candled to determine the viability and location of the embryo.

[1] Aided in part by a grant from Eli Lilly and Company.

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The fork of two large blood vessels on the C.A. membrane is located, and a circular area in the crotch is marked on the shell with a lead pencil. This area is disinfected with 95% alcohol, and a small circle ground through the shell with a dental engine equipped with a fine corborundum disk. Care must be taken not to damage the shell membrane. The circle of shell is removed with fine-pointed forceps, leaving the shell membrane intact.

The eggs are then inoculated by injecting 0.05 ml of the washed bacterial suspension just under the shell membrane and onto the C.A. membrane by means of a tuberculin syringe equipped with a ½-in., 27-gauge needle. The exposed shell membrane is then covered with sterile, melted paraffin-vaseline mixture, and the eggs are returned to the 37° C incubator.

The inoculated eggs are examined daily by transillumination, and embryos which appear to be dead are removed from the shell and examined grossly. The typical appearance is marked engorgement of blood vessels, and hemorrhage in the embryo, C.A. membrane, and sometimes in the yolk membrane. The fluids are usually clear, but the amniotic fluid may be tinged with hemolyzed blood. Cloudy fluids or embryos showing evidence of decomposition indicate contamination.

Toxigenic strains of *Coryn. diphtheriae* kill the embryos with remarkable uniformity. Ninety-nine per cent of 424 embryos were dead in 93 hr under the conditions described here. On the other hand, the majority of the embryos inoculated with nontoxigenic strains survived; i.e., 84% of 84 embryos were alive at 96 hr. Survival to the date of hatching gave essentially the same results (0.24% survival for embryos inoculated with toxigenic strains, and 81% survival for those inoculated with nontoxigenic strains).

A total of 32 strains was tested by this method. Of these, 9 were known *Coryn. diphtheriae*, and 23 had been isolated from cases of clinical or suspected diphtheria. Toxigenicity, as determined in guinea pigs, corresponded exactly with the results in the embryos. From these results, it appears that death of 80% or more of the embryos in test lots of 10 or more for each culture may be regarded as a specific indication of toxigenicity.

The response of the 9–10 day chick embryos is sufficiently uniform that it could be used as a preliminary method of differentiating between toxigenic and nontoxigenic cultures suspected of being *Coryn. diphtheriae*. The potential usefulness of this method lies in the fact that it may be used in small laboratories or in those not having provisions for the care and maintenance of animals.

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A Reevaluation of Steroid Nomenclature

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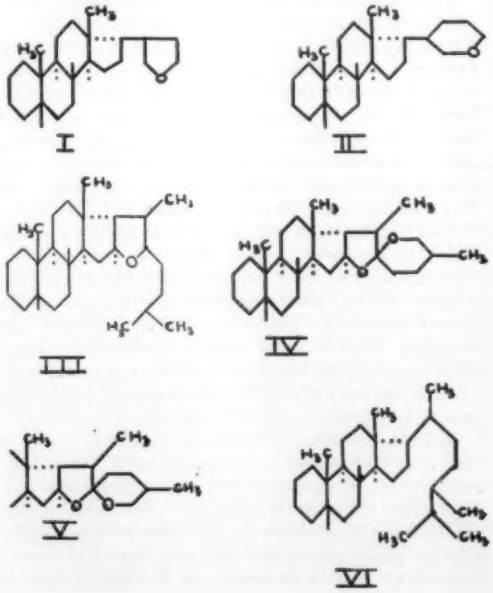
It must be obvious to those actively engaged in steroid studies that the various nomenclatures currently in use are by no means consistent and are not particularly facile in operation. The difficulties are noticeable when unnatural or unusual compounds are encountered, especially those differing in bridgehead configuration, those that have larger or smaller rings than usual, those that have one or more opened rings, and the genins, sapogenins, and their transformation products.

To attempt to bring some sort of order to this rather bewildering picture, a synthesis of the naming systems commonly used by authoritative workers has been made, which, with certain modifications, seems to lend itself to an adequate delineation of most compounds encountered in steroid chemistry.

As an initiation into the system, it is suggested that the parent substances commonly met with be named as follows: estrane,[1] androstane, and etiocholane; pregnane and 5-allopregnane; genan (I) and the 5-allo derivative, cholane and 5-allocholane; *E*-homogenan (II) and the 5-allo derivative, cholestane and coprostane; pseudosapogenan (III) and the 5-allo derivative, sapogenan (IV); 22-isosapogenan (V) and the 5-allo derivatives, ergostane and copro-

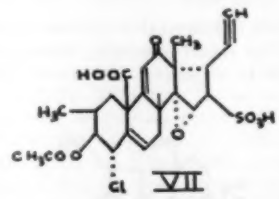


[1] Configuration undetermined at positions 5 and 10.

ergostane (VI); stigmastane; and coprostigmastane.

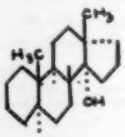
In naming substituted steroids, few modifications of current practice are suggested, but it is believed that the changes outlined will make for increased clarity. When double bonds are present, the symbol Δ should be used, followed by the usual cipher for locating the double bond. Only the lower figure necessary to fix the bond is employed, unless the bond culminates at a position that is not consecutively higher, in which case the two numbers necessary to fix the bond follow the Δ and are separated by a colon. In the event that a triple bond is part of the system, the symbol τ precedes the cipher. This new symbol should prevent confusion when double and triple bonds are present in the same compound. The usual "ene-yne" suffixes follow the name of the parent compound.

It is recommended[2] that the hydroxylic, epoxidic (ethereal, oxiranic), carbonylic, carboxylic, sulfonic, of other acidic functions shall follow, in that order, the name of the parent compound, each substituent being preceded by an appropriate number and the symbol α or β to indicate configuration in the conventional manner (*1*). Halogenic, aminoidal, alkyl, and aryl functions shall precede the name of the parent substance with appropriate number and symbol to indicate location and configuration, the substituent with the lowest number appearing first, followed by consecutively higher-numbered substituents. Thus, the systematic name for compound VII is 2β-methyl-4α-chloro-$\Delta^{5,9:11}$-τ^{20}-pregnadienyne-3β-ol-14:15-epoxide-12-one-19-oic-16β-sulfonic acid 3-acetate.[3]

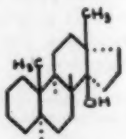


VII

If substituents are introduced at position 5, no further characterization other than the appropriate number is required, since the name of the parent compound is adequately descriptive. A substituent introduced at other bridgehead junctions requires no further characterization if the stereochemistry is normal. If, however, the substituent occupies a sterically "unnatural" position, the name of the parent compound is prefixed by a cipher locating the position, followed by the prefix "iso." Thus VIII is named androstane-14-ol, whereas IX is called 14-isoandrostane-14-ol (not androstane-14β-ol, or 14-alloandrostane-14β-ol).

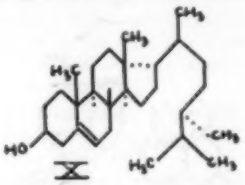


VIII



IX

Since it is known that stereoisomerism may occur in the side chain at positions 17, 20, and 24:28, the same rule may be applied as above. Campesterol (X) is named systematically as Δ^5-24-isoergostene-3β-ol (not Δ^5-24a-ergostene-3β-ol).

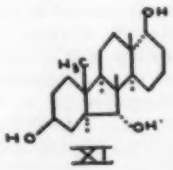


X

At nonbridgehead positions, or at positions adequately covered above, the usual α or β symbols may be used to denote configuration. The recommendation of Fieser (*1*) that hydroxylic substituents at position 20 be designated as "a" or "b" (rather than α or β) should be followed until spatial studies indicate actual configuration at that point. There seems to be little reason to deviate from the "iso" nomenclature in the case of carbon substituents in the side chain.

There is little justification for the use of "cis-trans" (referring to configuration at positions 3 and 17 with reference to positions 5 and 13) or "epi" designations. The former seems to be dropping from use, though the latter may have a certain degree of utility in the case of compounds of unknown or unproved structure, where "epi" may be used to indicate a configuration isomeric with that of the first-discovered material.

"Nor" and "homo" prefixes are used in the usual sense. If, as in XI, ring B has undergone contraction and ring D is expanded, the ring letter, followed by "nor" or "homo," is fully descriptive. In the event that a carbon atom in a side chain is missing, as, for example, in the 18-methyl group in XI, the numeral of the missing group, followed by "nor," is sufficient. Systematically, XI is named B,18-bisnor-D-homoandrostane-3β,6α,17aβ-triol[4] (not B,13-bisnor-D homoandrostane-3β,6α,17aβ-triol).



XI

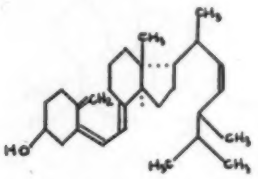
Nomenclature of steroids with open rings has long presented a problem, and most representatives of this class are saddled with trivial names.[4] It is recom-

[2] The suggestions in this paragraph violate certain of the Geneva rules, but there seems little reason to change the system used by many steroid workers.

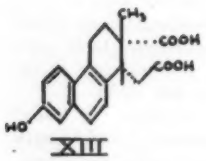
[3] May be named as 14:15-α-epoxide, but not required.

[4] It is realized that compounds of this class may be named according to the Geneva rules or those of the Ring Index (*2*). However, adherence to these rules may cause obscuration of the significant relationships to the parent steroids.

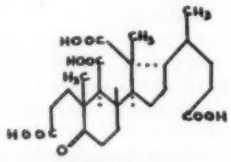
mended that Jaffe's (*3*) suggestion of using the prefix "seco" to indicate an open ring be adopted. "Seco" is to be preceded by the two numbers, separated by a colon, indicating the point of disjunction and is to antecede the name of the parent compound. Thus, calciferol (XII) is named 9 : 10-seco-$\Delta^{5,7,10:19,22}$-ergostatetraene-3β-ol; cis-bisdehydromarrianolic acid (XIII) is 16 : 17-seco-$\Delta^{1,3,5:10,6,8}$-14-isoestrapentaene-3-ol-16,17-dioic acid; and prosolannelic acid (XIV) is 3 : 5,11 : 12-diseco-4-norcholane-5-one-3,11,12,24-tetroic acid.



XII



XIII



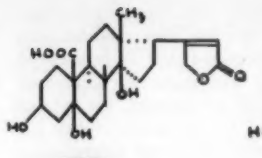
XIV

The current nomenclature in the realm of genins and sapogenins is most confusing. The suggestions outlined in the earlier part of this article should aid in bringing some sort of order to this area. Thus, strophanthidinic acid (XV) is to be called $\Delta^{20:22}$-14-isogenen-3β,5,14-triol-23-one-19-oic acid; anhydrodihydrostrophanthidin (XVI) is Δ^{14}-genen-3β,5-diol-19-al-23-one; isoperiplogenin (XVII) is 14-isogenan-3β,5-diol-14 : 21-epoxide-23-one;[5] episarsapogenin (XVIII) is sapogenan-3α-ol; and gitogenic acid (XIX) is 2 : 3-seco-5-allo-22-isosapogenan-2,3-dioic acid.

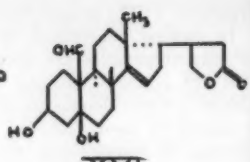
The pseudosapogenins, dihydrosapogenins and dihydrostrophanthidin (XVI) is Δ^{14}-genen-3β,5-diol-ilar ring structures, are named as pseudosapogenans.

The steroidal alkaloids represent an incompletely solved structural problem, with indications that a considerable number of these compounds are similar to solanidine. It is suggested (*4, 5*) that the parent compound be named solanidan (XX). For conformity with other parent compounds, it is indicated that

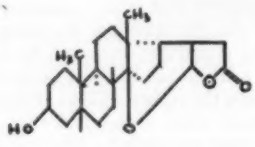
[5] May be named as 14 :21-β-epoxide, but not required.



XV



XVI



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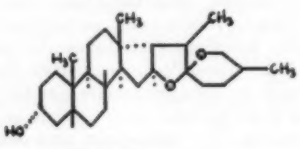
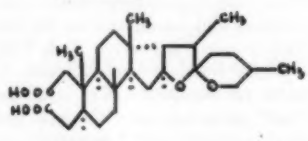
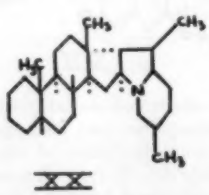


XVIII



XIX

the compound with a normal (coprostane) orientation at position 5 be called solanidan, whereas the compound similar to cholestane be named 5-allosolanidan.[6]



XX

It is obvious that the naming system proposed is cumbersome. It is one that is useful in most applications, however, and one that is reasonably consistent. It is believed that its utility outweighs its unwieldiness, and its routine application, therefore, is suggested.

[6] This contravenes Fieser (*5*), who has named XX as 5-iso-solanidan, and has reserved the name solanidan for the 5-allo compound.

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Comments and Communications

The American Naturalist

The American Naturalist, a journal established in 1867, became, beginning with its 85th volume in January 1951, the journal of the American Society of Naturalists, which has now assumed full editorial control. The society has designated L. C. Dunn, of Columbia University, as managing editor, and an editorial board is in process of formation. At present it consists of G. E. Hutchinson, Yale University; Thomas Park, University of Chicago; G. L. Stebbins, Jr., University of California; and Conway Zirkle, University of Pennsylvania. Jaques Cattell will continue as publisher, and the journal will be issued six times a year by the Science Press, of Lancaster, Pennsylvania.

The policy of the journal will be to continue its service to general biologists and naturalists through the publication of general essays, addresses, and symposia of biological societies which in the opinion of the editorial board contribute substantially to the purpose of the society in "correlating the various biological sciences."

It will continue to publish papers reporting new research, giving preference to those in which the chief interest inheres in theoretical interpretation and synthesis. It will make a special effort to publish promptly brief reports of new research, comments, and criticisms of material published in *The American Naturalist* or elsewhere, especially when these can be put in the form of concise letters to the editor. It will welcome such papers from naturalists in any country. Intending contributors are invited to consult the current number of the *Naturalist* (January 1951) for style and form to be used in preparation of manuscripts. Manuscripts and editorial correspondence should be addressed to: Editor, The American Naturalist, Box 2, Schermerhorn Hall, Columbia University, New York 27.

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Intracellular Fat Deposits in the Kidney

The recent paper by Dallemagne, Gerebtzoff, and Philippot (*Science*, **112**, 148 [1950]), regarding fat deposits in the kidney of the dog, may have called forth some comment from workers in the field.

The presence of fat in the renal tubular epithelium of the dog has been known for many years. The best recent description that I know is the work of Foote and Graffiin (*Anat. Rec.*, **72**, 169 [1938]), who studied teased preparations of the kidney.

Any statement regarding toxic effects upon the canine kidney manifested by changes in fat content would have to demonstrate significantly larger amounts than are present normally, or would have to demonstrate the presence of fat in a location other than the proximal convoluted tubules.

The photomicrograph shown by the authors (*op. cit.*) does appear to demonstrate more fat than one usually sees in the normal animal, but the diet of the animals would have to be carefully controlled in order to be certain that this is not a normal variation.

Their concluding statement, "This specific intracellular fat deposit in the kidney is bound to the still unelucidated biochemical lesion induced by hexachlorocyclohexane," is not justified.

The problem can be further elucidated by the use of other experimental animals, but in the light of present knowledge, the presence of fat in the dog's kidney must be regarded as other than "specific."

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Measurements in Colloidal Systems

A brief article under a similar title, by Jenny *et al.*, in the August 11 issue of Science raises questions of great importance in colloid chemistry. The customary electrochemical approach is abandoned by these authors. Starting from a theoretical consideration of the liquid junction potential arising between a salt bridge and a colloidal system, they proceed to discard entirely Donnan potentials between colloidal systems and true solutions in contact with them, and finally even to deny that analytical evidence for a Donnan situation exists in their experiments.

This is indeed a radical departure. Must chemists abandon the whole of Donnan theory? Is the evidence for it so feeble that it can be overturned by raising questions as to the elimination of liquid junction potentials by a saturated KCl bridge? The answer is obvious. Something must be wrong with these authors' interpretation of their own experiments. A detailed consideration of the latter is thus called for. Fortunately the article, although highly condensed, is sufficiently informative for this purpose. The pivot of discussion turns on potentiometric and analytical observations in a cell of the type

Sat. Calomel	K^+ Colloid	KCl	Sat.Calomel
Sat. KCl	+ KCl		Sat. KCl
		Y	

According to Donnan theory, the chemical potential of the molecular species KCl should, at equilibrium, be the same on both sides of the membrane, or boundary, Y. Since, however, the K colloid is not diffusible, this condition can only be met when the activities of chloride ions on the two sides are different. This difference in activity can then be used to calculate, by the Nernst equation, a boundary potential. Numerous ex-

periments in the literature have shown that this calculated potential agrees very well with the measured total potential of the above cell.

In the work here under consideration the statement is explicitly made that the chloride ion concentrations were the same on both sides of *Y*, yet potential differences were recorded. The key to this anomaly may be found in the following sentence: "Even after long standing, the Cl concentration of solution removed by rapid filtration from the right-hand compartment was identical with that of the solution in the left-hand compartment." Here the authors reveal a fundamental misunderstanding of the requirements of the Donnan situation. The Donnan theory is based on the postulate that the ionizing colloid is constrained in some way from attaining uniform distribution throughout the whole system. The nature of the constraint is immaterial. It can be furnished by a membrane or a filter, or by centrifugal or gravity fields of force, or by internal forces within a gel. What the authors do in performing a filtration in order to sample the colloid + salt system is simply to provide a different constraint. Therefore, what they get by filtration in the early stages will, of necessity, be identical with the solution that came to equilibrium across a membrane.

Their experimental result, therefore, is in precise agreement with Donnan principles. In order to test the matter in which they are interested—namely, whether the chloride ion concentrations on both sides of the membrane are in accord with the observed difference of potential—they should proceed differently. A sample of the colloid + salt system should be removed, the total chloride determined, and allowance made for the volume of solid phase present. In this way a valid chloride ion concentration may be obtained. This is the accepted procedure, and the authors have gravely misled themselves by departing from it.

There is thus absolutely no warrant for the rejection of Donnan theory, and we may now proceed to examine their other experiments with this in mind. The results presented in Table 1 of their paper, derived from a consideration of the cell

Sat. Calomel Sat. KCl	KCl A_2	Cation exchanger + KCl	KCl A_1	Sat. Calomel Sat. KCl
		X	*Y*	

are extremely interesting. At the outset they assume that the KCl in contact with the cation exchanger is, on the one side, at the same concentration as A_1, although the Donnan equilibrium precludes this. They now make a distinction between the two boundaries *X* and *Y*, stating that they believe a potential to exist at *X*, but not at *Y*. The present writer fails to see any fundamental difference between *X* and *Y* in this respect. When the KCl on one side becomes highly concentrated, then we believe that the corresponding potential becomes small.

The measurements represent, of course, the algebraic sum of the two potentials. This cell is, in fact, similar to one utilized by Mukherjee and Marshall (paper presented at the Colloid Symposium, June 1950) in testing the membrane functions of gels and pastes, based on theoretical considerations discussed earlier by Marshall (*1*). Through use of this approach, a plug of ionizing gel can be regarded as equivalent to a charged membrane. Below a certain value, measurements made using different cation activities on the two sides will show agreement with the ordinary Nernst equation; above this value the considerations brought forward by Meyer and Sievers (using Donnan principles) become pertinent (*2*). Actually the results in Table 1 are exactly as one would expect from this. The exchange material ion-*X* gives good selective membrane characteristics as regards cations up to an activity of 0.005, possibly somewhat higher, since the cell which employs 0.044 KCl against 0.0046 gives a potential within 4 mv of the Nernst value.

This raises the question as to the validity of the equation utilized in calculating transport numbers across a single liquid junction between a colloidal system and a salt solution. Since no details of the assumptions or the integration are given, little can be said, except to point out that Meyer and Sievers showed that, by the employment of Donnan principles, the relevant transport numbers could be calculated. Thus they opened up an approach which, when we deal with highly concentrated and highly ionized colloidal systems, may enable us to correct the observed results for the potential existing at the junction with the KCl bridge. So far this has not been done, but it should not be lost sight of, now that concentrated systems of highly ionized exchange resins are available for experiment. The main value of the paper under consideration is that it draws attention to this need. With a saturated KCl bridge, so long as the ionic activity of the cation associated with the colloid remains below 0.1, it is not likely to amount to more than a trivial correction.

One further comment regarding potentiometric measurements with glass membrane electrodes on highly concentrated systems is pertinent. The results, as Dole (*3*) has pointed out, depend both on the pH difference on the two sides of the glass and also on any difference in the chemical potential of the water. In very stiff pastes or gels the latter factor may well be of importance.

In conclusion, therefore, the experiments conducted by Jenny *et al.* in no way warrant the rejection of the Donnan principles or the further conclusions arising from this rejection.

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2. Meyer, K. H., and Sievers, J. F. *Helv. Chem. Acta*, **19**, 649, 665, 987 (1936).
3. Dole, M. *The Glass Electrode*. New York: Wiley (1941).

C. E. Marshall

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Book Reviews

The Birds of Greenland, Part I. Finn Salomonsen; illustrated by Gitz-Johansen. Copenhagen, Denmark: Einar Munksgaard, 1950. 157 pp. and 17 plates. $9.00.

This beautifully printed and illustrated volume is a welcome addition to the literature of Greenland ornithology. The text is in parallel columns of Danish and English. Common names of birds are given in Danish, English, and Eskimo. The introduction gives brief historical data and a short account of climate and life zones. Since it is planned to deal in detail with breeding species and regular visitants only (casuals are to be listed in an appendix later), the present volume includes 2 loons, 2 petrels, 1 swan, 6 geese, 8 ducks, 1 merganser, and 1 cormorant. Seventeen of these 21 are illustrated with a full-page color plate, each usually depicting a single bird, more artistically drawn than scientifically accurate and showing the bird in characteristic habitat.

The text gives much historical data on changes in status, also distribution, reproduction, ecology, food habits, uses of the birds to Greenlanders, and some taxonomic comments. These matters are treated in simple and direct fashion. The bibliography for all volumes will appear in the last of the series.

RALPH S. PALMER

New York State Museum

Ionization Chambers and Counters. D. H. Wilkinson. New York: Cambridge Univ. Press, 1950. 266 pp. $4.50.

This book, like others in the new group of *Cambridge Monographs on Physics*, will be eagerly awaited by nuclear physicists here and abroad. The publishers are to be congratulated on the content and appearance of their growing list in this series.

Several books devoted to the detection of nuclear particles by gas-filled ionization devices have appeared in the postwar years—none shows a greater unity than the present volume. From the first the author stresses the fundamental principles of counter action in their modern form. In a subject which has bordered on the occult this unity is refreshing, and one does not miss the older half-explanations too much.

In the main the book treats the theory of counter and ionization pulse formation and rather less constructional practice and application. It is thus truly a monograph, not an encyclopedia, and enjoys both the advantages and defects of limitation. Within the limitations the treatment is largely complete; the many aspects of initial and secondary ionization processes which lead to the observed pulse or current are analyzed and presented for practical cases and in graphs. The sections on proportional counters and ionization chambers are particularly exhaustive and present much new information and interpretation. The chapter on Geiger counters is also well done, but because of their wider use the omission of operational considerations is more evident. The reviewer particularly regretted the omission of gamma-ray counter efficiency, which might have been included, and of the electronics of pulse amplification, which would have overextended the volume. The author does, however, indicate clearly the requirements for good amplifier design.

The book will be most useful to students of counter operation, but its appeal to the numerous workers in tracer and radiation physics is likely to be rather limited. The former group includes many nuclear physicists; here they will find a much-needed comprehensive account of their principal tool of research.

ROBERT BERINGER

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Yale University

Morphology and Taxonomy of Fungi. Ernst Athearn Bessey. Philadelphia: Blakiston, 1950. 791 pp. $7.00.

The phenomenal growth in the importance of fungi in medicine, genetics, food manufacture, and other fields has forced many people to acquire more than a nodding acquaintance with this form of life. The discovery of many new genera and species, with numerous revisions in their classification, has made it necessary for Dr. Bessey to prepare this comprehensive volume—a completely revised and enlarged edition of an earlier book.

This text and reference book presents a straightforward and systematic outline of the fundamentals of mycology. After a brief introduction in which definitions, history, rules of nomenclature, and general mycology are discussed, Bessey develops the morphology and taxonomy of each class in a uniformly critical and informative manner. In each chapter the characteristics of the class are first given, followed by a key to the orders in that class. The orders are then discussed in detail and amply illustrated by concise line drawings. The chapter concludes with a key to the more important families and genera and a complete listing of the literature cited in the description of the orders. In this manner each chapter covers in detail one or more orders with remarkable industry and thoroughness.

Chapter 17 is outstanding as a critical review of the theories concerning the phylogeny of the fungi.

One may readily forgive Dr. Bessey his conservatism for including the myxetozoa in the text—even though he is of the opinion that they are more closely related to the animal kingdom—and for including with the Fungi Imperfecti a number of forms which he recognizes as perfect fungi.

No attempt was made to delve into medical mycology, the biochemistry, physiology, and genetics of fungi, or the production of antibiotics: ". . . . this book must not be expected to be a complete encyclopedia of fungi, but rather the foundation upon which to lay the various superstructures of the edifice of Mycology."

The book concludes with a guide to the literature for the identification of fungi. We are first given instructions on procedure based on the author's half century of experience in botany. The literature lists which follow are superbly organized and should be easy to use. The book is very well indexed.

One cannot commend this classic handbook too highly. Extremely useful for beginners, it has much to offer the advanced mycologist.

GEORGE P. CHILD

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Albany Medical College

The Private Life of the Protozoa—And of Their Neighbors, the Metazoa and the Insect Larvae. Winifred Duncan. New York: Ronald, 1950. 141 pp. $3.00.

This is an incredible book. That it got written is perhaps not astonishing, because people with an itch to write are likely to perpetrate almost anything. But that it got published passes understanding. I can think of no way of conveying the flavor of the book except by quotation, for which purpose the thrilling account of malaria transmission will serve admirably:

> This final knock of fate started from that quiet spore which had dropped, some time back, into the pool, from the foot of a canyon wren, and had sunk into the mud.
>
> Late in July, when the rains were in full swing, a young mosquito larva found this spore and ate it. The spore remained quiet until the larva climbed up out of the water and became a winged mosquito, very dainty and charming. The spore then woke up, divided itself into a male and female cell, and the male fertilized the female. Out of this was born a tiny, worm-like creature, pointed at one end, which bored its way right through the mosquito's stomach, got into its blood stream and finally landed in its salivary glands. On this journey it had rounded up and burst again into a lot of little spores.

You can guess the rest. One night when the mosquito was flying happily about it happened to run into Mr. Mac, and to plunge its proboscis into Mr. Mac's arm, where it found a lot of nice, red blood. But it also squirted in a lot of baby spores, and the baby spores loved the delicious, round beefsteaks in Mr. Mac's blood, and pretty soon Mr. Mac got chills and fever. Honest!

The author points out in her preface that she is writing "in the narrative form usually reserved for fiction" and making an "experiment in biography" which "required that I take certain liberties." So the book, apparently, is intended for amusement rather than instruction. But I find the purple prose tiring, even aside from its lack of any connection with the workaday world of facts. The book is liberally illustrated with line drawings, which I presume are intended to be "artistic," since the author states that "no attempt has been made to show the actual relation in size between the animals and the plants on which they are found." The drawings may or may not be good art, but they are certainly bad natural history.

MARSTON BATES

Riverside, Connecticut

Scientific Book Register

The Life of Vertebrates. J. Z. Young. New York: Oxford Univ. Press, 1950. 767 pp. $8.50.

Color Atlas of Pathology. Prepared under the auspices of the U. S. Naval Medical School. Philadelphia-London: Lippincott, 1950. 546 pp. $20.00.

Fluorine Chemistry, Vol. I. J. H. Simons, Ed. New York: Academic Press, 1950. 615 pp. $12.00.

Pharmacological Basis of Penicillin Therapy. Karl H. Beyer. Springfield, Ill.: Thomas, 1950. 214 pp. $4.50.

Textbook of Modern Pollen Analysis. Knut Faegri and Johs. Iversen. Copenhagen, Denmark: Einar Munksgaard, 1950. 168 pp. Dan. cr. 16.00.

Ion Exchange Resins. Robert Kunin and Robert J. Myers. New York: Wiley; London: Chapman & Hall, 1950. 212 pp. $4.75.

The Burden of Diseases in the United States. Alfred E. Cohn and Claire Lingg. New York: Oxford Univ. Press, 1950. 129 pp. with accompanying charts. $10.00.

Muscular Contraction: A Topic in Molecular Physiology. W. F. H. M. Mommaerts. New York-London: Interscience, 1950. 191 pp. $4.20.

Superfluids: Macroscopic Theory of Superconductivity, Vol. I. Fritz London. New York: Wiley; London: Chapman & Hall, 1950. 161 pp. $5.00.

An Outline of Scientific Criminology. Nigel Morland. New York: Philosophical Library, 1950. 284 pp.

Bacterial Polysaccharides: Their Chemical and Immunological Aspects. Martin Burger. Springfield, Ill.: Thomas, 1950. 272 pp. $6.00.

Textbook of Organic Chemistry. Louis F. Fieser and Mary Fieser. Boston: Heath, 1950. 741 pp. $6.00.

Diseases of Cereals and Grasses in North America. (Fungi, except smuts and rusts.) Roderick Sprague. New York: Ronald, 1950. 538 pp. $7.00.

Methods in Food Analysis Applied to Plant Products. Maynard A. Joslyn. New York: Academic Press, 1950. 525 pp. $8.50.

Sourcebook on Atomic Energy. Samuel Glasstone. New York: Van Nostrand, 1950. 546 pp. $2.90.

The History of Muscle Physiology: From the Natural Philosophers to Albrecht von Haller. Acta Historica Scientiarum Naturalium et Medicinalium, Vol. VII. Eyvind Bastholm. Copenhagen, Denmark: Einar Munksgaard, 1950. 256 pp. Dan. cr. 24.00.

The Fresh-Water Algae of the United States. 2nd ed. Gilbert M. Smith. New York: McGraw-Hill, 1950. 719 pp. $10.00.

Technological Applications of Statistics. L. H. C. Tippett. New York: Wiley; London: Williams & Norgate, 1950. 189 pp. $3.50.

Raudon-Cirujano Poblano de 1810. Aspectos de la Cirugia Mexicana de Principios del Siglo XIX en Torno de Una Vida. J. Joaquin Izquierdo. Mexico, D. F.: Ediciones Ciencia, 1949. 299 pp. $5.50.

News and Notes

1950 Meeting of Geological Society of America

David M. Delo

American Geological Institute, Washington, D. C.

THE 63rd annual meeting of the Geological Society of America was held in the Statler Hotel, Washington, D. C., November 16–18. Meeting with the G.S.A. were the Paleontological Society, the Society of Vertebrate Paleontology, and the Mineralogical Society of America. Host organizations were the Geological Society of Washington, the Paleontological Society of Washington, the Petrologist Club, and the Pick and Hammer Club. A total of 1,974 registrants was in attendance.

The meeting was preceded by two days of field trips, which took several hundred visiting geologists to Bear Island, the Chesapeake Bay area, and the Harpers Ferry district. These trips were led by staff members of the U. S. Geological Survey and The Johns Hopkins University. The afternoon of November 17 was devoted to excursions to laboratories and scientific research institutions in the Washington area. A lighter note was contributed by the annual extravaganza of the Pick and Hammer Club, entitled the "Last Days of Pomp," or "Jason's Golden Fleecing," which played in Lisner Auditorium on November 17 and 18 to capacity houses.

On the more serious side, three medals for distinguished research in geological science were awarded to one Canadian and two U. S. scientists. The Penrose Medal of the Geological Society was presented to Morley Evans Wilson, of the Geological Survey of Canada, for his basic and extensive contributions to deciphering the geology of the Canadian Shield. The Day Medal of the Geological Society was presented to Francis Birch, of Harvard University, for his geophysical research into the properties of materials at high pressures and high temperatures. The Roebling Medal of the Mineralogical Society was presented to Norman L. Bowen, of the Geophysical Laboratory, Carnegie Institution of Washington, for his quantitative experimental physicochemical work on the origin of igneous rocks.

One hundred forty-two papers were given at the technical sessions, in addition to a number of special forums and symposia. The address of the retiring president of the Geological Society, W. W. Rubey, U. S. Geological Survey, entitled "The Geologic History of Sea Water," was given on Thursday evening at the annual smoker, and created widespread interest. An open forum on problems in engineering geology and a symposium on the geology of coal also attracted attention among geologists devoted to the applications of the science to industrial problems.

A special session on Friday morning was composed of three papers in areas relating to geology; these were given by outstanding authorities in collateral scientific fields. Ernst Mayr, of the American Museum, spoke on the "Bearing of some Biological Data on Geology;" Harold C. Urey, of the Institute for Nuclear Studies, University of Chicago, offered "Evidence for a Primitive Homogeneous Earth;" and Percy W. Bridgman, of Harvard, outlined "Some Implications for Geophysics of High-Pressure Phenomena."

Chester Stock, of the California Institute of Technology, was elected president of the society for the coming year but died suddenly on December 6, 18 days after taking office. First Vice President T. S. Lovering will function as acting president until the Council chooses a successor to Dr. Stock. Officers of the society for the coming year are:

Past President: William W. Rubey
First Vice President: Thomas S. Lovering
Second Vice President: Marland P. Billings
Third Vice President: Charles E. Weaver
Fourth Vice President: George Tunell
Secretary: Henry R. Aldrich
Treasurer: J. Edward Hoffmeister

Councilors, 1949–51:
John W. Ambrose
S. W. Muller
Thomas B. Nolan

Councilors, 1950–52:
E. F. Davis
J. Hoover Mackin
Richard J. Russell

Councilors, 1951–53:
Ronald K. DeFord
Theodore A. Link
Ward C. Smith

The 1951 meeting of the Geological Society and associated societies will be held in Detroit, Michigan.

Scientists in the News

Bausch & Lomb Scientific Bureau's new director is **Howard S. Coleman,** former head of the Optical Research Laboratories, University of Texas. The directorship has been unfilled since the death of **Wilbur B. Rayton** in 1946. During World War II, Dr. Coleman spent several weeks at Bausch & Lomb on an assignment from Frankford Arsenal concerning inspection and performance of optical gunfire control instruments.

The National Jewish Hospital, Denver, announces the forthcoming retirement of **Harry J. Corper,** who

for the past 30 years has been director of research. During this period, the Research Department published many original investigations in the field of tuberculosis and received numerous scientific awards. In recognition of the position that has been attained the board of directors proposes to increase the budget and enlarge the facilities devoted to research. Plans also are being developed for recognition of Dr. Corper's services.

The associate director of the National Bureau of Standards, **Eugene C. Crittenden,** has retired after 41 years with the bureau. He has been a pioneer in the measurement of light and in the establishment of absolute standards for electrical units.

The National Bureau of Standards received the following visitors from abroad recently: **Marc de Hemptinne,** professor of physics, University of Louvain; **Jose M. M. J. Fripiat,** lecturer on physical chemistry of soils at Louvain and assistant at the Institut National pour l'Etude Agronomique du Congo Belge; and **Herbert L. Sichel,** statistics research officer, National Institute for Personnel Research, South African Council for Scientific and Industrial Research, Johannesburg.

Ralph A. Galbraith, chairman of the Department of Electrical Engineering at Syracuse University, has been appointed dean of the L. C. Smith College of Applied Science. Dr. Galbraith succeeds **Louis Mitchell,** who has retired.

The newly elected chairman of the American Chemical Society's Division of Organic Chemistry for 1951 is **William S. Johnson,** of the University of Wisconsin. He succeeds **R. W. Bost,** University of North Carolina. **Robert C. Elderfield,** Columbia University, was named chairman-elect, and **Nelson J. Leonard,** of the University of Illinois, was reelected secretary-treasurer.

Meetings

The **Indian Science Congress** and the **Pan Indian Ocean Science Congress** held concurrent meetings in Bangalore January 2–10. Approximately 2,000 delegates from the U. S., Canada, the United Kingdom, France, and Oceania attended the sessions.

The **Pakistan Association for the Advancement of Science** held its third Pakistan Science Conference at Dacca January 10–14. "Food and Nutrition in Relation to Environmental and National Health," "Energy in Service of Man," and "Development of Sea and Fresh-Water Fisheries in Pakistan" were subjects of the general symposium.

The **American Physical Society** held its 302nd meeting December 28–30 at the University of California, Los Angeles. Prospective meetings of the society are scheduled to be held at Columbia University, February 1–3; at Pittsburgh, March 8–10; and at Washington, April 26–28.

The **Association of Geology Teachers** organized a new Eastern Division in Washington, D. C., on November 18. This is the first of the regional divisions to be created under the expansion program of the association adopted at the annual meeting last April. Officers of the new division are: president, Ralph Digman, Harpur College; vice president, C. E. Prouty, University of Pittsburgh; and secretary-treasurer, Milton T. Heald, University of West Virginia. The parent association was founded in 1938 by teachers of geology in Middle Western institutions and now includes representatives of more than 50 departments in that general area. Membership of the Eastern Division is drawn primarily from New York, New Jersey, Pennsylvania, Maryland, Delaware, the District of Columbia, Virginia, and West Virginia, but is not restricted to this region. The objectives and activities of the association are concerned primarily with the accumulation and distribution of information relative to geological curricula, to aid in the dissemination of geological knowledge to the public, and to provide a clearinghouse for discussion of problems of geologic teaching in the interest of increasing its effectiveness. Teachers of geology who are interested in securing further information should write to Milton T. Heald, Department of Geology, University of West Virginia, Morgantown.

The 36th annual meeting of the **National Council of Geography Teachers** was held November 24–25 in Chicago. Members from 32 states and Canada attended. More than 79 colleges and universities in 27 states were represented. At the general sessions a variety of papers covered topics on both content and teaching problems. The official U. S. delegation reported on the Unesco International Seminar on the Teaching of Geography as an Aid to International Understanding held in Montreal last summer. The afternoon sectional meetings centered on three themes: geographic education, high-school and college geography, and mathematical geography. The last-mentioned and a section on conservation were planned jointly with the **Central Association of Science and Mathematics Teachers,** who were in session at the same time. The 1951 meeting will be held in Pittsburgh, Pa., during the Thanksgiving weekend.

Grants and Awards

Two **March of Dimes** grants, totaling approximately $47,000, have been made to University of Chicago scientists for research on infantile paralysis. One of the grants will be used in a search for a simple diagnostic test for polio, under the direction of Robert Rustigan, assistant professor of bacteriology; $23,440 has been allocated for his work on a complement fixation-type test. The other grant—$23,650—is to con-

tinue previous allocations to Earl A. Evans, Jr., chairman of the Department of Biochemistry, and his associates, who are engaged in research that has the ultimate aim of determining a fundamental basis for a chemical treatment for polio.

The American Ceramic Society has named John C. Hostetter, retired Corning Glass Works scientist, as winner of the **Albert Victor Bleininger Award** for 1950. Dr. Hostetter was director of research and development at the Corning Glass Works at the time the 200-inch mirror was cast for the Mt. Palomar telescope.

The treatment of extensive third degree burns will be investigated with the aid of a **PHS grant** under the direction of Vinton E. Siler, of the Department of Surgery, University of Cincinnati. Special target of the investigation will be an improved method for removing "slough," or dead tissue, from burned skin areas. Dr. Siler will investigate the removal of damaged tissue by the application of organic compounds such as enzymes. The grant was one of 110 made to 68 institutions and totaling $823,876. The fields of biochemistry, nutrition, metabolism, and genetics are covered by the grants.

The **American Farm Bureau Federation's gold medal award** for distinguished service to agriculture was presented during the 32nd annual meeting of the federation. The recipients were Donald F. Jones, chief geneticist at the Connecticut Agricultural Experiment Station, the late Ransom E. Aldrich, who was president of the Mississippi Farm Bureau Federation for 12 years, and Herbert J. King, who has fostered farm education in Wyoming.

Fellowships

First awards to be made from the **Chapman Memorial Fund** will be allotted this year. Applications for fellowships should be addressed to Chapman Memorial Fund, Department of Birds, American Museum of Natural History, New York 24. Applicants should state their training and experience and describe the nature and scope of the proposed project, as well as the plan of procedure, previous work on the project, and the amount of financial aid required. The names of one or more sponsors should be included. Any phase of ornithological research will be considered, and approximately $2,000 will be available for the first grants. Any necessary collections made during the tenure of a Chapman fellowship will be deposited in the American Museum. Applications must be received before *June 1.*

The **Department of Biochemistry, University of Washington,** has announced the availability of several graduate teaching and research fellowships. Candidates with a Bachelor's or Master's degree in chemistry, who wish to receive training for the Ph.D. degree in biochemistry, should submit their applications before *March 1* to the Executive Officer, Department of Biochemistry, Division of Health Sciences, Seattle.

Ten fellowships supported by the **Graduate College of the University of Illinois** will be awarded to qualified scholars who contemplate academic careers in the medical and allied sciences. Stipends of $1,800–$2,400 are available for medical and dental graduates, with a maximum of $3,000 for unusually well-qualified applicants. Pharmacy graduates receive a $1,200 stipend for the calendar year, and exemption from tuition fees is provided for all appointees. Registration for work toward M.S. or Ph.D. degrees is required. Applicants need not have completed clinical internships. Minimum requirements are: Bachelor's and M.D. degree; Bachelor's and D.D.S. degree; Bachelor's in Pharmacy and M.S. degree. The fellowships provide an opportunity for research experience in the basic sciences and the application of these sciences to clinical investigation. Applications must be received by *February 15,* with selections to be announced by *April 1.* Forms may be obtained from the Associate Dean, Graduate College, University of Illinois, 808 S. Wood St., Chicago 12, Ill.

Colleges and Universities

A new treatment for tuberculous empyema has been developed by the Department of Research Surgery of **Ohio State University College of Medicine.** It utilizes the digestive action of crystalline trypsin, supplied by Armour Laboratories under the brand name Tryptar. The solution is used to wash out the chest daily for 7–10 days. The enzyme liquefies the pus and debris, which can then be aspirated.

The University of Michigan, in conjunction with General Motors Corporation, has announced the establishment of a $1,500,000 research project to promote better health for its 446,000 employees, as well as other industrial workers. The Institute of Industrial Health at Ann Arbor will have as its objective research, education, and service in industrial medicine, health, and safety.

Duke University has just received a grant of $105,000 from the Liggett & Myers Tobacco Company which will support a three-year program of investigations involving research on basic chemical and plant science problems of potential interest to the tobacco industry. Members of the Duke Departments of Chemistry and Botany will supervise the project.

NRC News

The Chemical-Biological Coordination Center of the National Research Council has announced the publication of the third volume in its series of reviews concerned with chemical structure-biological activity relationships. CBCC Review No. 3, ***Histamine Antagonists*** (122 pp., $1.50), by Frederick Leonard and Charles P. Huttrer, discusses the pharmacological properties, methods of *in vitro* and

in vivo evaluation, methods of synthesis, structure-activity relationships, mechanism of action, and clinical application of histamine antagonists. More than 1,000 compounds, tested for potential antihistaminic activity, are listed in 48 classified tables, including structures, activities, and literature references. Emphasis is placed on the analysis of structure-activity correlations within the different classes of compounds, and on generalizations that can be deduced. The review contains 224 references. Copies may be obtained from the Publications Office, National Academy of Sciences, 2101 Constitution Ave., Washington 25, D. C.

The closing date for filing applications for **Fellowships in Tuberculosis** has been extended to *February 1*. These awards, administered by NRC for the National Tuberculosis Association, are designed to promote the training and development of investigators in this field. They are open to citizens of the U. S. who are graduates of American schools and who, as a rule, are not more than thirty years of age. Preference will be given to applicants who have the M.D. or Ph.D. degree. Only in exceptional circumstances will a fellowship be awarded to an applicant without either of these degrees. Inquiries should be addressed to the Secretary, Medical Fellowship Board, NRC, 2101 Constitution Ave., Washington 25, D. C.

The ninth edition of the directory ***Industrial Research Laboratories of the United States*** (445 pp., $5.) is now available from the National Research Council. The book contains information on the research laboratories of 2,845 industrial and commercial organizations, including the names of the president and research executives, the location of the laboratories, the numbers and kinds of research scientists and other personnel of the laboratories, the fields of research activity, whether any consulting services are offered, and the size of the laboratory library. The directory contains 2 appendices, one giving the same information for government laboratories which cooperate with industry, and the other listings of universities and colleges that offer research services to industry, including the kinds of facilities available. The book also contains a geographical index and a complete subject index of research activities. It may be ordered from the Publications Office, NRC, 2101 Constitution Ave., Washington 25, D. C. Checks should be made payable to the National Academy of Sciences.

Deaths

The president of the American Cyanamid Company, **William Brown Bell,** died of a heart attack December 20 at Marrakeech, French Morocco. He was on a trip surveying the company's interests abroad. Mr. Bell was responsible for the integration of the numerous companies that merged to form the American Cyanamid Company.

Jacob L. Snoek, head of the Physics Department of Horizons Incorporated, and formerly with the Philips Incandescent Lamp Works, Eindhoven, Holland, was killed in an automobile accident December 3 near Elkhart, Ind. Author of the book *New Developments in Ferromagnetic Materials,* Dr. Snoek was considered a world authority in the general field of magnetism.

Abraham Wald and his wife were killed in an airplane crash in southern India recently. Dr. Wald, who was professor of mathematical statistics at Columbia University, had gone to India to lecture at the Indian Statistical Institute of Presidency College in Calcutta.

Helen Corbin Monchow, land economist and former professor at Northwestern University, died recently at the age of 52. A former managing editor of *The Journal of Land and Public Utility Economics,* she was with the Housing and Home Finance Agency at the time of her death.

The developer of the tracer bullet, **William H. Buell,** died recently at 73. He was vice president and general manager of the Polychem Corporation at the time of his death.

Esther Rosencrantz, retired associate professor of medicine at the University of California's Medical School and a tuberculosis specialist, died December 18 at 74. She had recently given her collection of the work of Sir William Osler to the University of California.

The National Society for Crippled Children and Adults and the Alpha Gamma Delta International Women's Fraternity are sponsoring a four-week training course for employment and placement counselors working with cerebral palsied and other severely handicapped workers March 12–April 6. It will be given at the Institute of Rehabilitation and Physical Medicine of the New York University-Bellevue Medical Center under the auspices of the School of Education of the university. Training will include case studies and evaluations, clinical study and observation, lectures on medical and psychosocial aspects of the problem, counseling tools and techniques, placement tools and techniques, and vocational training. Counselors will also have an opportunity to visit rehabilitation and treatment centers. The purpose of the training program is to encourage public and private agencies to increase their services for the severely handicapped. Counselors from these agencies, as well as schools and industry, will be selected for fellowships according to their qualifications and contributions they can make in effective counseling and placement work. Applications must be received by *January 26*. Blanks and further information may be obtained from the National Personnel Registry and Employment Service of the National Society for Crippled Children and Adults, 11 S. La Salle St., Chicago 3.

Excerpts from a series of pupillography photographs taken on Kodak Infrared Film by Dr. Otto Lowenstein of Columbia University.

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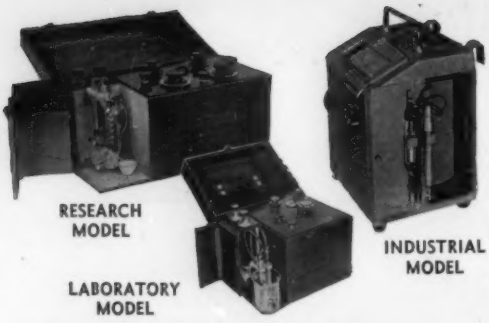

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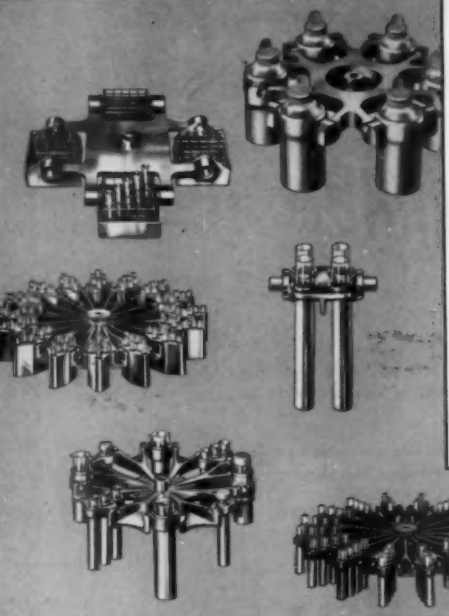

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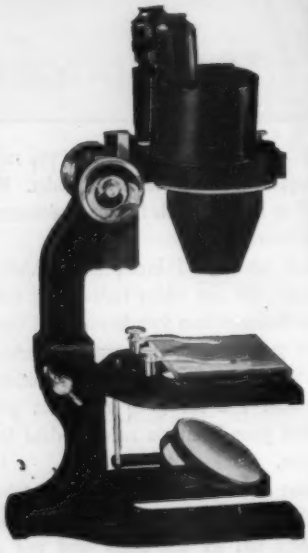

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